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Party Systems and Political System Performance: Voting Participation, Government Stability and Mass Violence in Contemporary Democracies

G. BINGHAM POWELL, JR.
University of Rochester

This article examines alternative visions of "strong" party systems by analyzing relationships between party systems and several dimensions of performance of the political process in 28 democracies of the 1967-1976 decade. Party system theorists agree that voting support for extremist parties manifests a weakness in the party system. They disagree, however, about the virtues or vices of party majorities and close linkages between social groups and parties. The evidence, including multivariate analysis of party system types and characteristics, with controls for environmental conditions, indicates that during this period extremist party support was associated with executive instability and mass rioting. Scholarly concern about other aspects of party system strength or weakness should focus on the desired feature of political performance. The representational, *multiparty systems were most successful in limiting rioting.* Aggregative majorities, responsible majorities, *and* representational *party systems all had good executive stability in the short run, although the first two types seemed somewhat more stable over the decade.* Aggregative majority *party systems were associated with low citizen voting participation.*

Social scientists and political commentators express increasing concern about the apparent decline in the strength of political parties in many contemporary democracies, including the United States. Their concern is founded on the widely shared assumption that a strong party system is indispensible to a healthy modern democracy. The importance of the party system for the achievement of various political process goals has been stressed by such influential theorists as Almond (1960), Duverger (1954), Huntington (1968), Leiserson (1959), Lipset and Rokkan (1967), Neumann (1956), and Sartori (1976). Yet, empirical studies of the relationship between the strength of the party system and a good performance by the democratic political system are rare.

The present analysis attempts to sketch the major alternative ideas about "strong" party systems found in the writings of party theorists, and to test them with cross-sectional data on party systems and political process performance from some 30 nations. The analysis considers all independent nations of over one million persons that seemed to have both competitive elections and enfranchisement of the majority of citizens for a five-year period before and during the late 1960s. (For a similar list of countries, see Dahl, 1971, appendices. Israel is excluded because of the large amount of externally based violence around and within the country.) By using virtually the full set of established contemporary democracies, and by examining several different aspects of performance of the political process, the analysis can provide clearer answers to some major descriptive questions raised in the literature on party systems: what type of party systems are in fact associated with citizen participation, stable and effective government, and the containment of political violence? Through multivariate analysis and some examination of trends over time, we can consider the more difficult and dynamic questions. The concern about party system "decline" can then be placed in theoretical perspective.

Strong Party Systems: Similar and Divergent Theories

Strong Party Systems: Agreement. Perhaps it is easiest to begin with areas of agreement about party system strength. Most students of parties and party systems see parties challenging the legitimacy of the regime itself, demanding some sort of fundamental change in the nature of the political order, as a threat to democratic regimes. Huntington (1968, p. 412) posits that a strong party system must be able to "moderate and channel the participation of newly mobilized groups in such a manner as not to disrupt the system." Duverger (1954, pp. 419-20) and Sartori (1976, Ch. 6) decry the "centrifugal" and "immoderate" nature of politics in countries in which such parties are strong. According to different theo-

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rists, "extremist" parties may include only those parties offering well-developed nondemocratic ideologies (such as communist and fascist parties); or may also include parties based on diffuse protest, alienation and distrust; or parties based on a proposal to break up the existing nation-state. Extremist parties have been the subject of some of the rare cross-national tests of the relationship between party systems and political performance: Taylor and Herman (1971) found that extremist party legislative strength was a major cause of cabinet instability; Dodd (1976) found party polarization related to reduced cabinet durability; Hibbs (1973) discovered that Communist party membership was related to higher levels of collective protest by citizens (riots and demonstrations). In general, substantial popular support for extremist parties is believed to threaten a democracy's ability to generate a stable government, to contain conflict within democratic bounds, to manage responsive and responsible transfers of power.

Strong Party Systems: Alternative Views. The theoretical views on strong party systems seem to diverge on two dimensions. On one hand, we find serious disagreement on the issue of the "number" of parties or "fractionalization" of the party system. One line of thought favors two-party, or majority-producing, or at least coalitionally dualistic, party systems. A contrasting line of argument favors nonmajority systems, with a number of parties representing a variety of views. On a second dimension, theorists disagree sharply as to the desirability of different parties being strongly and distinctively linked to different social groups and offering, typically, clear-cut alternative policy programs associated with those group interests. The intersection of these dimensions suggests the typology shown in Figure 1. Remarkably enough, serious and often impressive arguments have been advanced for considering, respectively, types A, B, B + D, and D as the strongest party systems and those having the most positive implications for political performance.

In the case of the *aggregative majority* party systems (Cell A) the idea is that the most desirable party systems should be dominated by a few parties, usually two, and that each party should appeal to a wide range of heterogeneous, cross-cutting groups. The parties are relatively similar to each other in their bases of social support, and the need to aggregate citizen majorities at election time drives them to converge to moderate center issue positions (Hoteling, 1929; Downs, 1957, Ch. 8). The basic similarity of the parties prevents elections from being too disruptive or threatening; the majorities can offer a basis for government authority and policy making; the internal diversity means that many groups can express interests through each party. The pressures to seek the electoral "center," as well as diverse internal cross-pressures and/or cross-cutting cleavages (Truman, 1951; Lipset, 1960, Chs. 3-4; Almond and Powell, 1966; Powell, 1976), prevent either majority party from moving to extremist issue positions. Hence these parties will offer policies pleasing to most citizens, or at least the citizen center; will shift policies flexibly; and should be relatively stable over time. This type of party system has been ably defended by such important theorists as Almond (1966), Epstein (1967), and Lipset (1960), as well as a long line of exponents of "pluralist" thought about American political parties.

The *responsible majority* type of party system (Cell B) combines majoritarian tendencies with clear-cut ties between parties and groups, the latter typically associated with parties offering programmatic policy differences and, usually, substantial centralization. This combination of party majorities and clearer party differences should en-

		Strong and Distinct Linkages between Different Parties and Social Groups	
		NO	YES
Majority-Producing	YES	Aggregative Majority A.	Responsible Majority B.
	NO	Fractionalized C.	Representational D.

Source: Formulated by the author.

Figure 1. A Typology of Candidates for "Strong" Non-Extremist Party Systems

sure strong, disciplined governmental authority and policy-making ability. Differences in the policy packages and group sympathies of parties in elections should help mobilize individuals and groups into politics and increase legitimate political activity. Moreover, the "responsible" party system increases citizen control over policy, as citizen majorities can choose between more clearly defined policy packages.

Simultaneously, the pressure on parties to moderate their policy proposals by seeking the winning median issue positions, will prevent serious policy differences from threatening regime stability. This idea of a strong, responsible majority system has long been admired by foreign observers of the British political system, especially Americans critical of their own parties' limited capacity to organize and mobilize citizen activity and to offer "meaningful" alternatives (Schattschneider, 1942; APSA Committee on Political Parties, 1950; Burns, 1963). At the same time, European theorists such as Duverger (1954, pp. 389, 415-19) and Sartori (1976, pp. 191, 292) have admired the centrist pressures that keep extremism in check, even while the clearer party differences mobilize support for the parties and their organizations.

On the other hand, defenders of the American system, such as Epstein (1967, esp. pp. 15-16, 355 f.) have criticized the rigidity and the imposition of artificial, simplistic majorities on a complex world that such systems seem to imply. Lijphart (1977a, 1977b) and Rokkan (1970) have pointed out the dangers of majority domination in conditions of intense group division, as in ethnically and religiously divided societies (see below). Moreover, those theorists suspicious of citizen mobilization into politics, and of party intensity, who see democracy as resting on elite respect for democratic rules and containment of antidemocratic citizen tendencies, are naturally concerned about the mobilization potential of these systems (Dye and Ziegler, 1975). Even Duverger (1954, p. 420) and Sartori (1976, pp. 292-93) note that if extremist parties do appear in strength, they can only be tolerated in multiparty, nonmajoritarian systems. Duverger argues that the centrist pressures should eventually moderate the positions taken by such parties in two-party situations, but that the democracy might not survive long enough for that to happen (1954, p. 420).

One of the most powerful statements about strong party systems and their implications focuses on the legitimate, mobilizing qualities of such systems, regardless of their number of parties, a type that would cover parties in both cells B and D. The best argument for *mobilizational* party systems is certainly to be found in the work of Samuel Huntington. In a precise and nearly complete contrast to the "cross-cutting," pluralistic ideas so ably presented by Epstein (1967), for example, Huntington argues (1968, p. 410) that total party domination of power-seeking in the society, and organizational depth and complexity, "particularly as revealed by the linkages between the party and socio-economic organizations such as labor unions and peasant associations" are prerequisites for political stability, at least in changing societies.

> A strong party system has the capability, first, to expand participation through the system and thus to preempt or to divert anomic or revolutionary political activity, and, second, to moderate and channel the participation of newly mobilized groups in such a manner as not to disrupt the system. A strong party system thus provides the institutionalized organizations and procedures for the assimilation of new groups into the system (Huntington, 1968, p. 412).

In his concluding analyses and empirical tests, it is true, Huntington argues that one- and two-party systems are likely to be stronger than multiparty systems in Third World countries. The strong multiparty system, he writes, requires "a one-to-one relationship between social forces and political parties." Because of the low levels of mobilization and participation in less economically developed societies, multiparty systems there tend to be fragile, fractionalized reflections of "differences of clique and family within a restricted elite" (p. 424). Such systems would be those in Cell C in Figure 1. Among the countries in this study, multiparty systems have, usually, the stronger linkages to groups. Hence, Huntington's (1968) ideas about multiparty systems with weak linkages cannot be well tested here, although the absence of such *fractionalized,* elitist systems is, of course, consistent with his emphasis on their regime instability. (For data on African systems of this type, see Jackman, 1978).

But the key theoretical question is that of mobilization; party-linked group involvement, not the number of parties as such, is critical. Huntington in fact directly refers to the evidence of turnout levels in the United States and Europe to argue the greater strength of parties in the latter nations (p. 402). In their discussion of the historical role of parties in Western Europe, Lipset and Rokkan similarly emphasize parties as "essential agencies of mobilization" (1967, p. 4). The suspicion with which such mobilization is viewed by many of the theorists discussed above need not be reiterated.

A final line of thought about strong party systems is worth exploring: the *representational* multiparty system, that combines strong linkages between parties and social groups with the absence of party majorities (Cell D, Figure 1). This line of argument has been developed most fully in the context of socially segmented societies, where the achievement of majority status by one ethnic or religious segment, for example, is highly threatening to the security and values of the other(s). Daalder argued that historically Dutch stability had been enhanced by the fact that none of the major subcultures, religious or economic, had at any point seemed likely to acquire majority control of the political system (1966, p. 219). Steiner (1974) emphasizes that in the Swiss context the multiplicity of segments prevents politics from taking on a dangerous "zero-sum" quality in which one segment's gains come at the expense of others. Lehmbruch (1974) and Jackman (1978, p. 1265 f.) have made similar arguments about deeply divided societies.

Lijphart (1977a) explicitly extends the argument to majoritarianism in the party systems, a point also implied in a number of the above discussions. He explicitly rejects the two-party or majority-duality argument as it applies to deeply divided societies. He argues, for example, that in postwar Austria the two-party system "was a strain on this overarching cooperation [of rival elites] rather than a support for it" (1977a, p. 62). Moreover, Lijphart emphasizes that in societies with deep social divisions, it is critical, too, that "the political parties clearly and separately represent all of the segments" (p. 64), and is dubious about the general "cross-cutting" arguments favored by many of the *aggregative majority* system theorists discussed above (p. 85 f.). It seems to him unlikely that parties successfully reduce segmented feeling by such cross-cutting alliances, and the absence of clearly defined representation of each group prevents the elites from negotiating settlements on behalf of their segments (p. 86). If the country is homogeneous, presumably social tensions are low and any party system can contain violence. If the society is divided, then a representational nonmajority system best sets the stage for the pattern of elite-negotiated accommodation that is the best hope of containing violence and regime instability in a democratic context. (Given the extended references to Lijphart's work, and the present finding that *representational* systems [strong linkages and nonmajorities] do perform very well, I should point out that the analysis here does not directly examine "consociational" theory. Consociational theory focuses on elite behavior, for which we have here no independent measures. Lijphart (1977a) discusses what I have here called *representational* systems as establishing conditions likely to favor elite accommodation. But not all *representational* systems are consociational, nor do consociational arrangements require *representational* party systems.)

Measuring Party System Strength: From Concepts to Measures and Types

My main purpose in this analysis is to test the proposals about "strong" party systems against some data on performance in contemporary democracies. In examining party systems we can proceed in two ways. One approach is to measure party system characteristics and use them to classify the party systems into types, corresponding to the theoretically derived ideal types in Figure 1. We can then examine the performance of each type, first in simple comparisons and then in multivariate analysis. This approach has the advantages of visual simplicity and continuity with the theoretical literature, which has usually discussed ideal system types. Consequently, the subsequent presentation uses measures of extremist party support, majoritarianism and the distinctiveness of ties between groups and parties to classify party systems, and then examines the relative performance of the different types.

However, the use of a typology, while attractive for presentational purposes, risks distortion and sacrifices valuable information. Such distortions can arise from creating some types that have only a few cases and from classifying systems using essentially arbitrary cutting points. The contemporary party systems manifest varying degrees of extremist voting support, party fractionalization, and distinctiveness of group support. Classifying them into types sacrifices much information about these relative differences. It is essential at least to supplement the analysis of party system types with an analysis of the theoretically suggested continuous variables upon which the types are based. Thus I summarize a regression analysis of these variables, with and without controls, by reporting the regression coefficients in Appendix 2. The regression results should reassure the reader that the use of various arbitrary cutting points to classify party systems does not bias the results. In fact, as one can see by comparing Table 4 and Appendix 2, the analysis based on continuous variables is more powerful in explaining the dependent variables than is the analysis of "types." (This point remains true if exactly the same number of cases and variables is used in both analyses.) The analysis of types is advantageous for discussion purposes, but gives up useful explanatory power.

Extremist Party Support. The first important property of party systems suggested by our theoretical overview is the degree of support given to extremist political parties. We shall begin by taking a broad view of extremist parties, defined as parties promising radical change in the social and political fabric. For the societies in question I have accordingly classified all the communist parties as extremist, as well as socialists in Chile and pacifist and far left parties in Ceylon, Norway, and Denmark; explicitly right-authoritarian parties, such as the Italian neofascists or strong man Jiminez's CCP in Venezuela are also classified extremist; so are the more diffuse antiregime, protest parties such as the Rural party in Finland, Progress in Denmark, and Komeito in Japan. Parties representing ethnic or religious positions that explicitly call for or imply alteration in the basic political structure, such as the Belgian linguistic parties in the 1960s through the early 1970s, are also classified as extremist. A full list is found in Appendix 1.

Obviously, these parties represent not only a wide range of issue positions, but also a substantial variety of positions regarding change in the political structure of the society. In a number of cases, such as the Italian Communists or the Japanese Komeito, there is substantial disagreement as to the "true" position of the party, both in intention and as viewed by voters and other parties. Moreover, theorists disagree as to whether or not diffuse, poorly organized, leader-oriented "protest" parties should be treated as equivalent to organized, ideological extremists for purposes of analyzing party systems. These differences can be of critical importance in the politics of individual countries. However, for present purposes, taking the average support for all of such parties in the elections of the mid-1960s to mid-1970s (three or four elections in most countries) will provide a measure of extremist support that most observers would probably roughly accept.

The choice of a cutting point between extremist "challenged" systems and supportive ones is somewhat arbitrary. The median country in this period had about 9 percent of the vote going to extremist parties in the average election. Some of these averages conceal substantial swings in extremist support. For party system classification, I shall use 15 percent as a mark of extremist support of a serious nature, a figure that seems roughly to correspond to the point at which observers of the country begin to express concern, and a level achieved by about one-quarter of the democracies. The exact averages of extremist voting support are shown in Appendix 1.

Majorities and Low Fractionalization. Intuitively, it would seem that the majoritarian or "two-party" property of party systems should be the simplest and most straightforward to measure. Yet, as Sartori (1976, p. 185 f.) has perceptively pointed out, the problem is sufficiently complex that observers have come up with quite radically differing numbers of two-party systems, when they take on the task of classifying contemporary party systems.

Here it is worth checking two measures. Were legislative majorities produced in half or more of the elections held during period in question? If so, we shall classify the country as majoritarian in tendency. I shall include the permanent pre-electoral coalition in Australia, but not other coalitions. If we compare this measure to an elegant and useful continuous measure, Rae's index of fractionalization, we see that they are strongly related. Rae (1967) proposed a measure expressing the probability that any two legislators will belong to different parties. The average fractionalization figures, shown in parentheses after the country in Table 1, correspond closely to the probability of majority outcomes. The higher numbers do help distinguish systems with only three or four parties from very divided multiparty systems. The multivariate regression in Appendix 2 uses the Legislative Fractionalization Index. (The Pearson correlation between extremist vote and extremist legislative seat representation is .97; the correlation between vote fractionalization and seat fractionalization is about .86. Thus, it does not, overall, make too much difference which score is used, although the legislative numbers are smaller.)

Linkages Between Groups and Parties. The concept of strong linkages between groups and parties, versus diffuse and heterogeneous bases of party support, is a less well-developed concept in party system theory. Some scholars, such as Epstein (1967), have emphasized their opposition to "programmatic" parties, while Duverger (1954) stresses centralization as a key property. Huntington (1968) and others have emphasized the key question of organizational depth and complexity. A quantitative measure that is likely strongly associated with organizational strength and programmatic consistency and difference, and is directly tied to the concerns about group representation, was invented by Alford (1963) in analyzing the distinctiveness of class voting and further explored by Lijphart (1971). In each country we divide the citizens into two major groups, each containing at least a third of the citizens. The groups are based on key demographic characteristics relevant to party politics in the country in question: occupation of family head, ownership of land, frequency of church attendance, religious preference, or ethnic origin. We can then compare, for example, the percent of workers voting for "left" parties with the percentage of nonworkers voting for these parties. Thus, in Sweden in 1964, 84 percent of the manual workers voted for the Social Democrats or the Communists,

Table 1. A Classification of Party Systems in 1965-75: The Nonextremist Party Systems

		Strong Linkages Between Groups and Parties	
Majority-Producing		NO	YES
	Yes–in half of elections	*Aggregative Majority* Canada (63) Costa Rica (58) Ireland (61) Jamaica (45) Philippines (49) Turkey (63) Uruguay (61) United States (48)	*Responsible Majority* Australia (61) Austria (54) Ceylon (67) New Zealand (49) United Kingdom (53)
	No–less than half of elections	*Fractionalized* Lebanon (95) Venezuela (72)	*Representational* West Germany (57) Netherlands (84) Norway (72) Sweden (69) Switzerland (81)

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974), *Keesing's Archives,* and Appendix 1.

Note: These party systems had an average vote for extremist parties under 15 percent for elections in this period. See Appendix 1 for parties classified extremist and for linkage scores. Linkage scores over 30 are "strong." The numbers in parentheses are legislative fractionalization scores.

Table 2. A Classification of Party Systems in 1965-75: The Extremist Party Systems

		Strong Linkages Between Groups and Parties	
Majority-Producing		NO	YES
	Yes–in half of more of elections	India (60) Japan (61)	
	No–in less than half of the elections	Chile[a] (73)	Belgium (76) Denmark (79) Finland (81) France (71) Italy (78)

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974), *Keesing's Archives,* and Appendix 1.

Note: These party systems had an average vote for extremist parties over 15 percent for elections in this period. See Appendix 1 for magnitudes of extremist vote and for parties classified as extremist, as well as linkage scores. Linkage scores over 30 are "strong." The numbers in parentheses are legislative fractionalization scores.

[a] Because of ambiguities in the survey, the linkage classification of Chile is somewhat uncertain and may understate the strength of linkage.

while only 32 percent of those from other occupations did so. The difference between these two percentages is the "index of class voting" for Sweden in that year: 52. In the United States, in contrast, in that same year 78 of the manual workers favored the Democrats, while only 61 percent of other occupations preferred them to the Republicans: yielding a class support index of 17.

The measure of linkages between groups and parties that we shall use here is the highest of the group indices in a given country (more frequently religion than any other cleavage). Where multiple survey results were available, these were averaged for the country in question. In most cases the linkage indices are quite stable over time, and correspond closely to less crude statistical measures of the association between demographic position and party preference. (See Powell, 1980, for more details and comparisons with other measures.)

The major problem with the index of party-group linkages is that survey data were not available for some of the less economically developed democracies between 1967 and 1976. I was able to locate fairly reasonable survey data for 23 of them, but not for Ceylon, Costa Rica, Lebanon, Turkey, and Uruguay. However, the secondary literature on their party systems is virtually unanimous in describing very weak linkages between groups and parties in each case except Ceylon, where race, religion, and intensity of religious preference were very important in the mobilizing efforts of the SLFP in the late 1950s, and 1960s. In Table 1, therefore, I have classified Ceylon as a strong linkage country, the others as weak linkage, and chosen 30 as the index's cutting point. Again, where available, the continuous measure does help discriminate more fully between countries with rather modest linkages, such as Germany, and countries with very strong ones, such as Sweden and the Netherlands.

Classifying the Party Systems. Tables 1 and 2 classify each of the party systems according to their average extremist voting support, their election of legislative majorities and the linkages between groups and parties in the period between the mid-1960s and the mid-1970s. Table 1 shows eight *aggregative majority* systems, five *responsible majority* systems, two *fractionalized* systems, and five *representational* party systems. The class of *mobilizational* systems, with strong linkages and nonextremist parties, would combine the responsible and representational types. Table 2 shows the eight *extremist* party systems, indicating their configurations of majorities and linkages, which are discussed below after considering *extremist* party systems as a single type. At various points in the discussion I shall refer also to majoritarian, as opposed to multiparty, systems and the former will include the systems above the horizontal center line in the tables, that is, the *aggregative* and *responsible* types.

Political Performance Measures: Participation, Stability and Violence

Dimensions of Performance. There is little doubt that some of the confusion surrounding the role of party systems derives from disagreement about what constitute desirable attributes of the political process in a democracy, and divergent theories about the consequences of popular participation, government stability, and so forth. No data that could be presented would resolve the underlying value differences separating some of the party system theorists. Therefore, the objective here is to examine three aspects of performance that have dominated much of the discussion of party systems, without claiming that they are comprehensive or trying to settle their relative importance.

Theorists are obviously sharply divided about the desirability of citizen involvement in politics. Participation is critical for Huntington (1968) and Schattschneider (1942), for different reasons, although it is of dubious or negative value to many *aggregative majority* theorists. The falling rate of voter turnout throughout the 1960s and 1970s is one reason for the concern many observers express about the decline in the American party system. Because of its great salience for many democratic theorists, and some major party system theorists, we cannot ignore voting participation as an aspect of desirable political performance.

Government stability and, more vaguely, effectiveness, has been a major theme in the debate over political party systems, as in the debate over electoral laws. Despite warnings by some analysts that we should not necessarily equate unstable cabinet government with poor policy performance, Anglo-American theorists have tended to reject multiparty systems largely because of a belief in their governmental instability. In his recent massive review of democratic political theory, Roland Pennock writes confidently that "under the cabinet system of government, the difficulty with multiparty systems is that no one party is likely to be able to form a Government. Coalition governments tend to be weak and unstable" (Pennock, 1979, p. 358). In a similar vein, Harry Eckstein (1968, p. 437) argued that "effective authority in a democracy must always rest on 'solid masses of steady votes' in the representative assembly." More cautiously, Robert Dahl's discussion of party systems and polyarchy concluded:

> Although it is exceptionally difficult to cast this proposition in operational terms, it seems clear that highly fragmented multiparty systems . . . can lead to unstable or weak coalitions that are unable to cope with major problems and thereby exaggerate in the eyes of the public and even the political elites the partisan or game aspects of political life. These results in turn may stimulate a loss of confidence in representative democracy and the willingness to tolerate political conflict (Dahl, 1971, p. 122).

Similar or even more extreme attributions of disastrous consequences to executive instability and/or lack of majority governments can be found easily enough (e.g., Bracher, 1964, p. 356). Without demonstrating the impact of government stability on measures of policy performance, which would require a substantial discussion in its own right, we can here examine the putative relationship between party system and stable government. The presumption of the inevitable instability of multiparty parliamentary systems has been challenged by some recent work (Dodd, 1976), and requires empirical investigation.

Finally, virtually all party system theorists are concerned with controlling or limiting political turmoil and violence. Some believe that violence is best controlled through stable government and firm executive authority. Others argue that mobilizing citizens and groups through legitimate and representative channels will more effectively inhibit political turmoil. But containing violence seems to be a goal mentioned by most party system theorists, even if they disagree about the means.

Ultimately, much of the concern about strong party systems in democracies has been for the preservation of the democratic regime. In the 1967-76 period, national democracy was overthrown in Chile, the Philippines and Uruguay, and was at least temporarily suspended in Ceylon, India, Lebanon, Turkey and, perhaps, Jamaica. Each of the countries experienced well above average numbers of deaths because of political violence before the suspension of democracy. All of them are less economically developed nations. Once one controls for level of economic development, the type of party system shows no relationship to regime durability or overthrow, although as a descriptive statement we can see that such replacements were more likely in the *aggregative majority* party systems. The statistical analysis needed to treat the dichotomous dependent variable is complex, and the negative conclusions do not seem to justify the space required for presentation of the results. Therefore, the presentation concentrates on deaths by political violence, a continuous dependent variable closely linked to regime durability in theory and practice. (See Powell, 1982, forthcoming, for a more extensive discussion of the relationship between party system and regime durability.)

These three aspects of political performance do not, of course, exhaust all the goals that might be sought in a democratic polity or demanded of an ideal party system. We have not considered specific policy objectives, such as economic growth or income equality, nor other process goals, such as the protection of individual freedoms. (See Eckstein, 1968, p. 437, and Pennock, 1979, Chs. 7-8. Almond and Powell, 1978, suggest a broad typology of "political goods." Closer to the present approach are Gurr and McClelland, 1971, although they do not include participation.) But it seems fair to argue that citizen participation, government stability and mass violence have been major concerns, explicitly and implicitly, in the debate about strong party systems and their virtues.

Measurement of Performance. The measures of performance, even for these three dimensions, are imperfect, but each has been the object of a substantial research tradition. For citizen participation I have collected data on the voting turnout of citizens of voting age, averaged over elections in the time period covered by the study. (The size of the voting-age population was drawn from the United Nations data. Variations in registration laws reduce the usefulness of the turnout of registered voters for comparative analysis. See Powell, 1980.) Although voting turnout is only weakly related to many other forms of citizen participation (Verba, Nie and Kim, 1971, 1978), there are both normative and empirical reasons for thinking that citizen involvement through the electoral process is a critical aspect of participation. Citizen participation in elections seems essential to citizen control of collective policies by government leaders and closely linked to the legitimacy of democracy itself. Moreover, the mobilization of poorer citizens, those usually less likely to participate through other channels, may have major consequences for both substantive government policies and the legitimacy of the democratic regime.

Government stability has been emphasized by many of the party system theorists. As one measure of governmental performance I have used the durability of the tenure of the chief executive—the president in a strong presidential system, the prime minister in a parliamentary system—in months, up to a maximum of 36 months. The 36-month maximum is used because it is the minimum period between regularly scheduled elections that can directly affect the executive in any of the democracies. If the chief executive dies or retires for health reasons, I have counted his tenure as unbroken, if normally replaced by a party member. If the party makeup of a parliamentary cabinet changes, or if the regime changes, I have counted the executive as changing, even if the same individual remains prime minister. (This procedure is fairly similar to that used by Dodd, 1976, for parliamentary systems. Also see the "maintenance of authority" measure developed by Gurr and McClelland, 1971.) As a second measure of government performance I have examined the proportion of time the party or parties in the government commanded a legislative majority, rather than occupying a minority status vis-à-vis the legislature. These government performance measures do not capture, of course, the diversity of policy problems addressed, nor the success of governments in either making or implementing authoritative solutions for them. The measures do provide crude indicators of possible government ability to develop and legislate policy in quite different types of political systems. Frequent changes in the control of the chief executive and/or frequent minority governments seem likely to make the formation and implementation of effective policy difficult, and often constitute a symbol of poor government performance to citizens.

As measures of violence, I have used the yearly average numbers of riots and of deaths due to political violence while the country was a democracy. These two violence measures capture the essence of the two major kinds of violence—often called "turmoil" or "collective protest," on one hand, and "internal war," on the other—that emerge in most cross-national violence studies (see Hibbs, 1973, Ch. 1). The data themselves were taken from Taylor and Hudson (1972) for the 1958-1967 period, the latest time covered by them, and collected from *Keesing's Archives* and *Facts on File* for 1967-1976. (The latter are obviously less reliable and complete than the former, and both will be examined separately in the data analysis.)

Testing the Predictions

1. The Association of Party System and Political Performance. Some theorists have simply asserted the probable impact of party systems without attention to other variables also affecting political performance. Others have been more cautious, suggesting various factors involved. No comprehensive descriptive studies have been presented. In this situation it seems appropriate first to describe party performance in terms of simple bivariate relationships. Which types of systems do show the most citizen participation, greatest stability and least violence? As even these simple descriptions have not previously been available, and the relationships have been the object of dispute, they are of intrinsic interest. However, after summarizing the simple descriptions, we shall turn to multivariate analysis that will allow us to control for probable confounding variables, and thus provide theoretically more appropriate tests of the impact of party systems on the measures of political performance.

Table 3 presents the evidence with which we can examine the simple, descriptive expectations and predictions. On the far left of the table are indicated the dimensions and measures of political performance. The table is divided into two parts. In the top half we see the median performance levels of the different types of party systems, beginning with the weak extremist type, three distinct candidates for strong systems, and finally the national average for comparison. The *mobilizational* type, suggested by Huntington (1968), would be the *responsible* and *representational* types taken together. The two *fractionalized* systems are not shown. In the lower half of the table are the simple correlations between the continuous measures of party system characteristics (extremism, legislative fractionalization, and party-group linkages) and the same performance measures. In the column at the far right is an estimated variable for linkages, as the measure could not be obtained in five nations, and they are thus excluded from the previous column.

Reviewing the expectations against the results in the table, we can quickly see that voting turnout fits our expectations well. The *aggregative majority* systems have below-average electoral participation. All of the other types have above-average voting turnout. Strong linkage between groups and parties is powerfully related to more participation of citizens at the polls. Fractionalization and extremism are both weakly but positively related to voter turnout. These relationships reemphasize the short shrift given to participation among "aggregation" theorists. They reconfirm, incidentally, the mobilizing effects of linkages between parties and groups, but do not indicate the relative importance of strategic or organizational bases (see Powell, 1980).

In the associations with governmental performance, we find the *extremist* systems indeed do poorly, as predicted by most theorists and as observed by Taylor and Herman (1971) and, assuming extremism to be an indicator of polarization, by Dodd (1976). The *extremist* systems have less durable executives, and more frequent minority governments. The *aggregative majority* systems show a surprisingly mixed picture: they have more durable executives, as expected, but the median level of control of the legislature is below average.

(We shall see in the next section why this is so.) The *responsible majority* systems are very effective in creating executive control, although their executive durability is only average. In looking at the individual countries, we find a tendency for leaders in these systems to call early elections to create majorities, rather than building government coalitions (for example, as in Austria in 1971 and Britain in 1974). The major surprise in looking at government performance, however, is the very good showing, on both durability and majority control, of the *representational* systems. This relationship does support Dodd's argument that under some conditions multiparty systems can be as stable as majoritarian ones (1976). The data here support the ideas of the *representational* theorists against those arguing for the necessity of majorities.

Table 3. Party Systems and Political Performance: Empirical Associations

	Median Performance of Party System Types				
	"Weak"	"Strong" Candidates			All
Political Performance Measures	Extremist (8)	Aggregative (8)	Responsible (5)	Representational (5)	(28)
Participation:					
Voting Turnout	78%	71	81	84	75%
Government:					
Executive Stability	24 mo.	33	29	36	30
Executive Control	79%	81	93	92	84
Violence:					
Riots 1958-67	2.8/yr.	2.3	1.8	.3	2.0
1967-76	2.2/yr.	1.3	.1	.3	.8
Deaths 1958-67	1.2/yr.	.8	.1	0	.3
1967-76	1.8/yr.	18.0	0	0	2.7
Suspension or Overthrow of Democracy 1967-76	25%	50	25	0	29

	Correlations with Party System Properties			
Political Performance Measures	Extremist Vote (28)	Fractionalization (28)	Linkage Score (23)	(Linkage Estimate) (28)
Participation:				
Voting Turnout	+.21	+.30	+.58**	(+.57**)
Government:				
Executive Stability	–.51**	–.41*	–.51**	(–.36*)
Executive Control	–.14	–.16	–.10	(–.19)
Violence:				
Riots 1958-67	+.29	–.28	–.47*	(–.31*)
1967-76	+.19	–.27	–.44*	(–.32*)
Deaths 1958-67	–.04	–.31*	–.61**	(–.43*)
1967-76	–.07	–.34*	–.53**	(–.42*)
Suspension or Overthrow of Democracy 1967-76	–.01	–.34	–.50**	(–.48**)

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974); *Keesing's Archives;* Charles Taylor and Michael Hudson, *World Handbook of Political and Social Indicators* (New Haven, Conn.: Yale University Press, 1973); and Appendix 1.

Note: Scores are medians, not averages. Violence scores are truncated to 90th percentile score to prevent extreme outliers from biasing correlations. Very similar results are obtained with log transformations of the dependent variable.

* = significant at .05.
** = significant at .01.

In the correlations between characteristics of party systems and government performance, we can see that extremism, fractionalization and party-group linkages are all negatively related to executive stability, and not much correlated to control. These negative correlations fit our expectations about extremism. But the general associations, in conjunction with what we have just been saying about the good performance of the multiparty, nonextremist systems, suggests a point that was emphasized by many of the *aggregation* and *responsible* party theorists: that multipartyism may encourage extremist party and/or polarizing tendencies over time. That is, where the multiparty systems can avoid extremist mobilization, they may often prove able to build stable governments. But it may be quite difficult to avoid the development of extremist tendencies in multiparty systems over time. From this perspective, the *representational* system argument fails to consider the long-run dynamic problem of polarization, such systems may be excellent. On the other hand, this dynamic and causal argument needs more careful consideration, to which we shall return. It could be that the simple correlations are just the result of the fractionalizing effect of extremist parties after they gain support.

In the key area of containing violence, of course, we have both common and divergent expectations. Almost all the theorists expected the *extremist* systems to do poorly, but arguments diverged on the performance of other system types. In the case of rioting, the expectations about *extremist* systems, and extremist voting, are realized, both for the more reliable data from 1958 to 1967 and the less reliable, but more temporally appropriate, data from 1967 to 1976. However, with regard to more serious deadly violence, the *extremist* systems were above the median in the early period, but below it in the latter one. The correlation between extremism and deaths was trivial in both periods. (Moreover, the *extremist* systems were no more likely to experience overthrow or suspension of national democracy than nonextremist systems in 1967-1976.)

Perhaps an even more significant surprise is the very poor performance of the *aggregative majority* systems on all the violence measures, with above-average incidence of riots and, especially, a more than average number of deaths. Here the simple associational evidence certainly supports the mobilizational argument of Huntington and the antimajoritarian ideas of Daalder, Steiner, Lehmbruch, and Lijphart. The *responsible, mobilizational,* and *representational* systems all do well at avoiding both types of violence. The record of the *representational* systems is particularly impressive. In the analysis of system properties, on the right of the table, legislative fractionalization is associated with *fewer* riots and deaths; strong party-group linkage is powerfully related to containment of violence and sustaining of democracy.

We cannot make causal attributions before we look at the impact of economic development, population size, and other environmental conditions. But several points are worth emphasizing here. First, it seems clear that extremism is, as expected, linked to poor government performance and to citizen turmoil, but less sharply to deadly violence and regime instability. Second, among the strong party system candidates, the arguments of the *mobilizational* and *representational* theorists have the best cases. If such systems can avoid extremism, they seem to indicate good performance on all the democratic measures we have looked at. Third, the patterns in the correlation analysis and in Table 2 indicate the need to look at dynamic properties of party systems as well as at simultaneously controlled relationships.

2. Controlling for Environmental Conditions. The initial examination of the associations between party systems and political performance does not take us very far in evaluating arguments about the effects of party systems. We know from common sense and from other studies that the social and economic setting may well help determine both party system and political performance. We need to control for such factors to see if the party system associations are spurious. Here we shall take account of environmental social and economic conditions and of the basic distinction between presidential and parliamentary systems. However, we shall not attempt here to untangle the independent effects of election laws and party systems, whose causal connections have been the subject of much theoretical dispute. (Causal modeling and multivariate regression analysis does suggest that incorporating election law variables into the analysis does not fundamentally alter the conclusions presented here. Where relationships between election laws, party systems, and these forms of performance do appear, it is the party system variables that seem to be more proximate to outcomes. A broad analysis of the role of party systems in linking social, economic and constitutional setting to political performance is presented in Powell, 1982, forthcoming.)

Table 4 presents a dummy variable regression analysis of the main types of party systems, adding a major control variable to see if the party system effects are reduced or changed. As in the previous table, the dependent variables, the measures of political performance, are shown on the far left. For each measure we then first see the standardized regression coefficients for the three distinctive types of "strong" party systems.

Although standardized coefficients are presented here, careful examination of the unstandardized coefficients does not change the present conclusions. Each type is entered in the regression equation as a simple dummy variable. *Extremist* systems are not entered, and are represented by the constant term (along with the two *fractionalized* systems). Thus the coefficients represent primarily the contrast between each type of strong party system and the *extremist* systems. The strength of each discrimination is suggested by the size of the coefficient.

This comparison of coefficients contains, basically, the same information that was contained in the left-hand part of Table 3. The second line following the dependent variable, however, shows the new values of the coefficients after a control for the most powerful of the environmental conditions that additional studies of these variables have indicated. Because of the complexities of analysis and the small number of cases, only one environmental variable, the most powerful, is shown here, but the results are not basically changed if the additional variables shown in Appendix 2 are also added. Thus, the second line indicates both the increase in multiple R caused by adding a major environmental condition, and how the coefficients of the party system types are changed.

If we look at voting turnout, we see that in the regression without controls, the *aggregative majority* systems have low turnout, while both *responsible* and *representational* types show moderately positive coefficients. As the *mobilizational* systems are simply those two types combined, they are not shown separately here. But

Table 4. Party System Types and Political Performance: Standardized Regression Coefficients, Adding Control Variable

Dependent Variables		Party System Dummy Variables			Major Control Variable		
Political Performance Measures	Multiple R	Aggregative Majority	Responsible Majority	Representative	Compulsory Voting	Population Size (log)	GNP/ Capita (log)
Voting Turnout	.41	–.31*	+.15	+.10			
(N = 28)	.56	–.19	+.22	+.17	+.40**		
Government							
Executive Stability							
All (N = 27)	.25	+.25	+.13	+.22			
Non-Presidential (N = 19)	.44	+4.0*	+.40*	+.40*			
Executive Control							
All (N = 27)	.31	+.09	+.33*	+.01			
Non-Presidential (N = 19)	.45	+.01	+.21	–.35			
Violence							
Rioting 1958-67	.40	–.26	–.23	–.44**			
(N = 26)	.83	–.15	–.10	–.31**		+.74**	
Rioting 1967-76	.31	–.11	–.14	–.35*			
(N = 28)	.79	+.03	–.03	–.23*		+.74**	
Deaths 1958-67	.31	+.07	–.09	–.28			
(N = 26)	.67	+.01	–.04	–.06			–.64**
Deaths 1967-76	.34	+.03	+.05	–.32*			
(N = 28)	.73	–.08	+.07	–.10			–.70**

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974); *Keesing's Archives;* Charles Taylor and Michael Hudson, *World Handbook of Political and Social Indicators* (New Haven, Conn.: Yale University Press, 1973); and Appendix 1.

Note: System types are coded 1 if the system was of that type in Table 1; otherwise, coded 0. Violence scores are truncated to the 90th percentile to prevent extreme outliers from biasing the regressions. Very similar results are obtained with log transformations of dependent variable.

* = F over 1.7 (significant at .1 in a sample).
** = F over 3.0 (significant at .05 in a sample).

their participation-mobilizing propensities are obvious. If we control for compulsory voting laws that provide penalties for less-involved voters, the negative impact of the *aggregative* systems is less pronounced, while the coefficients for the other types are made more positive. The compulsory voting variable is strong and significant. As compulsory voting is a legal requirement created by the government, one inference here is that *aggregative majority* systems are less likely to adopt such mobilizing regulations. In either case, the basic inferences here are not changed much, although our explanatory power is increased with attention to voting laws. Similar effects are found if we add registration laws to the analysis (see Powell, 1980).

In looking at government performance, we should undertake the controls differently. An examination of constitutional conditions rapidly shows the importance of the distinction between the presidential and parliamentary systems. The presidential systems are those in which the chief executive cannot be removed or replaced by simple majority action of the legislature. In most of these systems (Chile, Costa Rica, France, Philippines, the U.S., Uruguay, and Venezuela), the president is more or less directly elected and has a different base of support from that of the legislature. There is little doubt that in most of these countries the presidential system predates the development of the party system (except in France), and is not merely an effect of the type of party structure.

A major objective of presidential constitutions is to "divide" power, by making the president more independent of the legislature, but also weakening his possible control over it. In fact, the presidential systems succeed in both objectives by our measures here. Most presidents serve out their full term before leaving office; the median executive durability here was the 36-month maximum, although the United States fell short of that with the Nixon resignation, as did Uruguay, with military intervention limiting the authority of the civilian president in 1973. In Chile, Allende was overthrown before the end of his term, but after 36 months in office. Moreover, the circumstances of electing presidents from a minority party proved to be very common in the presidential systems (rather than an American aberration, as often thought). Even in the two-party systems of Costa Rica and the Philippines this was quite common. Median percent of the time the executive controlled the legislature, in the sense of being of the same party, was only 72 percent in the presidential systems, as compared to 85 percent in the nonpresidential systems.

These constitutional effects are quite powerful. To keep the presidential systems in the analyses of stability could create misleading results. Moreover, most of the theoretical discussion of the impact of party system on stability is explicitly directed to parliamentary systems. Hence, in Table 4 the presidential type systems (those mentioned, plus Switzerland, which has a collective executive that cannot be removed by simple legislative action; and Lebanon, in which the president is elected by the legislature for a fixed term) are out of the regressions on governmental performance. The result is to clarify sharply the impact of the party system. (Adding a dummy variable for those systems and using the full set of cases yields empirically similar results.) For executive durability, we can see that while the presidential systems were included, all the strong systems did moderately well, although without explaining too much variance in stability. After their removal, the explanation of relative durability increased markedly, and *each* of the strong party systems did *equally* and significantly well in comparison to the extremist systems. Such environmental variables as small population size and modernization level had no positive effect on stability, given the role of the party systems. It is interesting to see that the party system theorists are quite correct in seeing the negative impact of extremism, once we delete the residential systems, but that all the strong systems perform equally well vis-à-vis each other. The majority systems, whether *aggregative* or *responsible*, do *not* do better than the *representational* nonmajoritarian systems—as long as the latter avoid extremist representation. In creating executive control, however, the *representational* systems do less well, and the *responsible* systems have the greatest advantage, although neither coefficient is quite statistically significant. The touted advantages of majorities, especially majorities by parties strongly linked to groups, emerge in establishing executive control. It seems easier to elect a majority government than to build one. (An analysis of elections, coalition formation, and government stability, including use of the cabinet as a unit of analysis, is presented in Powell, 1982, Ch. 7, forthcoming.)

In looking at violence we see much more dramatic impacts of social and economic conditions. As Hibbs (1973) and others have shown, population size is a very powerful factor shaping rioting. Adding (log) population size greatly increases the power to explain rioting and diminishes the size of the party variable effects. Nonetheless, extremism continues to be associated with rioting, as can be seen from the negative coefficients for all the "strong" party systems. Moreover, we see here, again, that the *representational* systems continue to stand out as distinctively effective in avoiding riot behavior. The magnitudes are diminished by controlling for population size, but the relative advantage of these systems over the other "strong" party systems is about the same. The coefficients associated with *aggregative* and *responsible* systems are reduced to a very low level, but that for *representation* remains significant. (Nor would it be eliminated by adding variables such as level of economic development, average growth rate, ethnic fragmentation, or percent communist party membership.) As can be seen by comparing the *responsible* and *representational* systems, the effect of *mobilization* in general is less impressive with the controls.

In the case of deadly violence, the impact of controlling for environmental effects, especially GNP/capita, can be summarized very simply: they tend to eliminate any net effects of the party system dummy variables. In the dummy regression without controls, the *representational* systems do very well, the others about average. But once we control for level of economic development, the party system types have little effect on deaths by violence. Both the poor performance of the *aggregative* systems and the very good performance of the *representational* ones seem to have been an indicator of the level of economic development of the societies, rather than a direct factor in exacerbating or constraining violence.

In Appendix 2 these same phenomena can be examined by looking at the three major system properties in multiple regression, along with added controls for major environmental variables. The analysis clearly shows that party-group linkages are very powerfully related to voter participation. Extremism is the major predictor of executive instability, while fractionalization tends to lead to loss of executive control. Extremism is positively related to rioting with both sets of data, but fractionalization seems consistently related to *less* rioting. (Almost all of these fractionalized legislatures, it will be recalled, have strong linkages between groups and parties. With weak linkages, expectations might be different.) None of the party system variables significantly affects deaths by violence. In short, these data confirm the conclusions from the dummy variable regressions. However, they leave open the critical problem of the association between fractionalization and extremism, and its implication for political performance.

Representational Systems, Extremism and Performance

The analysis has been able to answer some questions, primarily the descriptive ones, but leaves others very much open. Perhaps the most theoretically important of these is that of the relationship between multipartyism, extremism and political system performance. The major problem is dynamic: the type of system we have called *representational* was identified as having multiparty nonmajorities, strong linkages to groups, and limited extremist support. These systems performed very well. But the *aggregative* and *responsible* party theorists have stressed the extremist propensities of competitive dynamics in multiparty systems. From their point of view, to argue the good performance of nonextremist *representational* systems is to miss half of the point, or to create an artificial "type" that will be unstable as soon as discontent develops in the society. These data cannot provide a conclusive answer to the problem, but some comments about indirect evidence are in order.

Cross-sectional Comments. Suppose we look at Table 1 from a different point of view and integrate the *extremist* systems from Table 2 into the typology. We see immediately that the *representational* systems are also more likely to be extremist than either the *aggregative* or *responsible majority* types. In fact, there is a substantial correlation between extremist voting and legislative fractionalization: r = .65, across the 28 political systems. The multiparty systems in general are more likely to have voting and representation of extremist parties. Even setting aside the multiparty systems with weak party-group linkages (Lebanon, Venezuela), the representational arrangements are often associated with extremism. So far, this association supports the *aggregative* and *responsible* system theorists. The fact that the *representational* systems also have, on average, stronger linkages between groups and parties also suggests the centrist pressures operating to some degree in the majoritarian countries.

On the other hand, even including the *extremist* systems, the multiparty systems still seem better able to limit rioting. The tendency of extremist parties to stimulate and to be associated with turmoil seems offset in part by the dampening effect of multiple, group-linked, nonextremist parties. It is illuminating to look directly at the types of rioting. In collecting the event data for the 1967-1976 period, I recorded whether or not party activity seemed to be involved in stimulating riots. Due to the confusions of riot events, as well as the limitations of my sources, these characterizations are not very reliable, but they are suggestive. The majority systems of all kinds were less likely to have party-related rioting. But the minority systems were much less likely to have riots that were not associated with any party activity. Of the eleven minority systems with strong party-group linkages, even including Chile and the other *extremist* cases, four had riots at the rate of one per 500,000 citizens: 36 percent. Not only did all these serious riot countries also experience at least 15 percent voting for extremist parties, but also reports suggest that in most of these riot countries extremist party activity was involved in stimulating at least some riots. In the majority countries, on the other hand, serious rioting levels were reached in 67 percent of them, although there was little or no party involvement in over half of these. The penalty of the minority systems, that they were more likely to develop extremist party activity that was associated with rioting, was offset by two facts: (1) not all extremist party activity was associated with rioting; (2) diffuse, nonparty-related rioting was much more frequent in the majority-dominated types (40 percent of them, as compared to 13 percent of the minority types, had purely nonparty riots).

A final point is also suggestive. Our analysis has treated all extremist parties as alike. Given some arbitrariness in classifying parties as extremist, there is even less certainty about subclassification. But it seems to be true, in comparison and regression, that support for extremist parties representing diffuse, intense protest was not, on average, associated with rioting. It was support for parties calling for a change in the nation-state and/or the democratic regime that was especially related to the outbreak of riots. As many of the extremist parties present or appearing in minority party systems are of the diffuse protest type, the examination of the type of extremist party also suggests that the *representational* party argument has some solid basis in fact, when it comes to the control of violence. However, representation of all the kinds of extremist parties shown in Appendix 1 seems to impede formation of stable executives.

Comments on System Change. The analysis was carried out on systems that had been working democracies for at least five years. Hence, both these nations and their party systems were for the most part relatively established. Nor surprisingly, there is great continuity in the party system types and characteristics between the 1960s and 1970s. Indeed, the correlations between our measures of fractionalization and of extremist support at points a decade apart are about .80 to .85. With such great stability, we cannot say much about party dynamics. But the changes that did take place and their consequences are suggestive for considering the problem of multipartyism, extremism, and performance.

In three countries, the level of extremism changes enough that party systems would have been classified differently in Tables 1 and 2 over the two decades. Japan, Belgium and Denmark developed substantial new extremist voting in the 1970s; the former would have been classified *aggregative,* the latter two *representational* in the earlier decade. Moreover, extremist voting in Netherlands, Norway and Uruguay passed 15 percent in at least one election, although it was not sufficient to bring the average over that figure. These changes suggest that the *representational* systems were, indeed, the least stable of the system types in making either temporary or continuing shifts in extremist voting support. This fact is consistent with the argument made by the *aggregative* theorists, and with the general association of fractionalization and extremist support in the cross-sectional analysis.

The increases in extremism in the multiparty countries do not, however, seem to be so much of the centrifugal type, in which all the parties flee the center, as reflections of party and constitutional arrangements in which previously unrepresented cleavages (Belgium, Uruguay) or dissatisfied subgroups (the right in Denmark) can gain easy entry into the political system. These arrangements encourage representation of extremism, and its stimulation in the sense that would-be leaders can be assured of some return with rather low levels of citizen support. There is little evidence here of existing extremist parties or previously pro-system parties moving to greater extremes. Nor is existing extremism significantly correlated with increases in extremism (+.16 between extremist vote in the early 1960s and its increase). Such systems seem to manifest an absence of centrist and consolidating pressures, rather than positively centrifugal ones.

The increased levels of extremism seem to have hurt government stability. Increases in the representation of extremist parties were associated with less executive stability in Denmark, to a lesser extent in Norway, Belgium, and the Netherlands, at least temporarily. Failures of control of the executive over the legislature were also evident in several of these nations. Thus, the good short-run performance of the *representational* systems in creating stable executives must also reckon with the apparent tendency of systems of this type to encourage future destabilizing representation of extremist parties. The net advantage for stable government would seem to lie with the *aggregative* and, especially, *responsible majority* systems.

While increases in extremist support seemed to lead to the expected negative consequences for governmental performance, the consequences for rioting were less clear cut. Extremist party growth in Denmark and Norway did *not* accompany substantially more rioting. In Japan, rioting seemed to decline at the same time that the Komeito and communists were gaining strength, perhaps due to the simultaneous moderation in the position of the socialists. On the other hand, the new issues raised by extremist parties in Belgium and Uruguay were associated with turmoil, and the growth of the neofascists in Italy and far left in

Chile was accompanied by more rioting. Thus, we may again see some association of extremism and riots, but party related rioting was often outweighted by other types in the majoritarian systems.

Conclusions

The writings on democratic party systems concur in characterizing party systems with substantial extremist party voting as "weak." But theorists disagree on the desirable attributes of "strong" party systems. Empirical examination of parties and political system performance in the late 1960s and early 1970s illuminates these disagreements. Whose arguments seem to have held up best? Even if we control for environmental conditions, the answer depends in part on the aspects of political performance we emphasize.

First, all of the theorists are correct in positing that support for extremist parties is related to several kinds of poor system performance. Extremist party representation is strongly related to executive instability. Voting for such extremist parties is also significantly associated with rioting, even after we have introduced controls.

Second, once we control for such environmental conditions as economic development level, ethnicity, and size, none of these party system types or their attributes were significantly related to deaths by political violence or regime suspensions. It needs to be reemphasized that the universe here consists of the democracies that did "work" for at least five years, and contains few of the purely elitist *fractionalized* systems with weak linkages discussed by Huntington (1968) and Jackman (1978).

Third, in the case of voting participation, the theorists' expectations seemed well realized. The *aggregative majority* theorists were frequently suspicious of citizen involvement, and these systems had low voter turnout. The systems with strong linkages between groups and parties had high turnout, on average, and those linkages explained more of turnout than any socioeconomic features. Whether mobilization was due to organization, group identity, or other factors is to be explored.

Fourth, in the case of rioting, all the theorists wanted turmoil limited and contained. The analysis suggests that the *representational* theorists had the best of the argument in these data. There is substantial support for the theory that nonmajorities, when coupled with strong linkages between groups and parties, help constrain riot activity and channel discontent through the party system. *Representational* systems—moderate, multiparty, strong linkages between groups and parties—performed very well on the riot measures. These systems did seem somewhat prone to develop extremist party activity, and sometimes such activity encouraged rioting, but the balance significantly favored the *representational* case. (If only fractionalization and [log] population size are entered in the regression, the former remains significantly negatively related to rioting in the 23 countries shown in Appendix 2.)

Fifth, in the case of executive stability, in the short run all the strong party system types did well (once account was taken of presidential constitutions). Party system and constitutional variables were more important than any environmental features here. As Dodd argues (1976), multiparty systems can be quite stable. The *aggregative, responsible* and *representational* systems all performed equally. However, as suggested by a number of theorists, multipartyism and extremism were associated with each other in cross-sectional comparison, and the multiparty systems were more likely to experience at least temporary surges of extremist representation. Where these surges occurred, executive stability was usually shaken. Thus the long-run pattern seemed to favor the *aggregative* and *responsible* systems in promoting executive stability. As expected, the *responsible majority* systems did have the best record on overall government performance, associated with both stability and executive control of the legislature. (Presidential constitutional arrangements, by the way, strongly tended to executive stability, but frequent minority governments, and seemed to promote *aggregative majority* party systems.)

These conclusions are based on data gathered over a limited period and on aggregate comparisons. Since the original work was done, I have been able to replicate the analysis of riots and deaths using the new data collected for the *World Handbook III* (Taylor and Jodice, 1981), rather than the data collected by me.[1] Recalculation of the dummy variable analysis yields coefficients that correspond almost exactly to those in Table 4. The regression coefficients corresponding to Appendix 2 for rioting 1967-76 (23 cases) are: Extremist Voting = +.52; Legislative Fractionalization = —.45; Party-group Linkages = —.10; Population Size (log) = +.59. Analysis also shows a significantly negative impact of legislative fractionalization on protest demonstrations, further supporting the argument that *representational* party systems help channel citizen discontent through the legitimate electoral channels and inhibit turmoil. (These results are for truncated dependent variables, as usual; log-transformed ones yield similar coefficients.)

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[1] I am grateful to David A. Jodice and Charles Taylor for permitting me to use these data for comparison before publication of the new *World Handbook III*. Appreciation is also due to Karl Deutsch and the International Institute for Comparative Social Research, Science Center Berlin, for support of data collection for the new *Handbook*.

It is particularly interesting that the conclusions concerning *representational* party systems and legislative fractionalization and their inhibiting effects on citizen turmoil are even more strongly supported with the data collected from a broader range of sources. Party system types and variables

Appendix 1. Party System Measures for 1965-1976

Country	**Extremist Party Support** **Average Percent Vote**	**Extremist Party Support** **Parties Classified Extremist**	**Legislative Fractionalization**[a]	**Party-Group Linkage Scores**[b]
Australia	0	--	61	33
Austria	1	Communists	54	49
Belgium	21	COMM, Language parties	76	50
Canada	0	--	63	28
Ceylon	13	COMM, LSSP, MEP	67	N.A.
Chile	30	COMM, Socialist, Far Left	73	25
Costa Rica	4	PAS	58	N.A.
Denmark	18	COMM, SPP, Left Soc, Progress, Single Tax	79	47
Finland	25	COMM, Rural	81	55
France	25	COMM, Extreme Left	71	34
West Germany	3	NPD	57	36
India	18	Jan Sangh, CPI, CPI-M	60	13
Ireland	0	--	61	21
Italy	37	COMM, PSIUP, Monarch, MSI	73	40
Jamaica	0	--	45	18
Japan	16	COMM, Komeito	61	24
Lebanon	5	--	95	N.A.
Netherlands	13	COMM, BP, PSP, GPU, D66 (1967,71 only)	84	64
New Zealand	1	Values	49	42
Norway	9	COMM, SPP, Lange, Soc. Electoral Alliance	72	40
Philippines	0	--	49	20
Sweden	5	COMM	69	46
Switzerland	8	COMM, Republican Movement, National Action	81	45
Turkey	9	Nat. Action, Turkish Labour, Nat. Salvation	63	N.A.
United Kingdom	3	Scottish, Welsh Nationalists	53	38
United States	0	--	48	20
Uruguay	12	FIDEL, Frente Amplio	61	N.A.
Venezuela	12	COMM, MAS, National Civic Crusade	72	13

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974) and *Keesing's Archives.* Extremism judgments are made by the author. Complete names of parties can be found in the sources. COMM is the abbreviation used here for communist parties. Linkage scores are based on surveys from Richard Rose, (ed.), *Electoral Behavior: A Comparative Handbook* (New York: Free Press, 1974) and other sources cited in G. Bingham Powell, Jr., "Voting Turnout in Thirty Democracies," in Richard Rose (ed.), *Electoral Participation: A Comparative Analysis* (Beverly Hills, Calif.: Sage Publications, 1980), pp. 16-17. Data for Japan were provided by Norman H. Nie and Sidney Verba. Tables for Venezuela were generously computed by Enrique Baloyra and John Martz.

[a] Average party fractionalization of the legislature following the elections. The score is the probability (multiplied by 100) that any two legislators are of different parties: $1 - (\sum_{1}^{N} T^2)$, where N = number of parties, T = party decimal share of seats. The formula is based on Douglas Rae, *The Political Consequences of Electoral Laws* (New Haven, Conn.: Yale University Press, 1967).

[b] Class, religious or ethnic party support indices (whichever is highest) based on averages of available surveys from 1964-75. Scores can range from 0 to 100. The index of, for example, class voting is the percent manual workers supporting leftist parties minus the percent of those in other occupations supporting leftist parties. Independents and Don't Knows are excluded.

continue to be insignificantly related to deaths caused by political violence. These confirmations are most reassuring as to data reliability, although they do not expand the time frame. I should add that all substantive results in the discussion also hold for the subset of 18 developed nations whose GNP/capita was over $1300 in 1972.

As I have tried to examine the full universe of "working" democracies of over one million citizens in the late 1960s, only a few new cases can be added to the analysis in the near future. But the analysis can and should be extended to different periods and to an examination of the intervening attitudinal and behavioral linkages that theorists believe connect the party systems and political performance in the democracies. The interplay of dynamic and static relationships and of party system, legal and constitutional arrangements, cultural background and environmental conditions is also worthy of more detailed examination than has been possible here. Other desirable attributes of the democratic process and its consequences can, of course, be investigated. It is already clear that concern about decline in party "strength" must focus on specific system performance criteria.

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Appendix 2. Party System Properties and Political Performance: Standardized Regression Coefficients, Adding Controls

Dependent Variables		Party System Variables			Major Control Variable		
Political Performance Measures	Multiple R	Extremist Vote	Legislative Fractionalization	Party-Group Linkages	Compulsory Voting/or Ethnicity (Violence)	Population Size (log)	GNP/ Capita (log)
Voting Turnout	.60	+.19	−.17	+.64**			
(N = 23)	.72	+.12	−.35	+.68**	+.43**	−.11	−.03
Government							
Executive Stability							
All (N = 23)	.68	−.54**	+.15	−.47**			
Non-Presidential (N = 17)	.87	−.70**	−.13	−.19	–	+.08	+.02
Executive Control							
All (N = 23)	.38	+.04	−.44	+.41			
Non-Pesidential (N = 17)	.47	+.14	−.62*	+.34	–	+.09	−.19
Violence							
Rioting 1958-67	.71	+.73**	−.68**	−.18			
(N = 21)	.92	+.46**	−.36**	−.02	–	+.74**	+.21*
Rioting 1967-76	.60	+.56**	−.50*	−.28			
(N = 23)	.83	+.27*	−.24	−.15	–	+.66**	+.07
Deaths 1958-67	.64	+.04	−.21	−.53**			
(N = 21)	.87	−.07	−.03	−.06	+.34**	+.33**	−.38**
Deaths 1976-76	.63	+.42*	−.48*	−.35*			
(N = 23)	.79	+.16	−.29	−.01	+.09	+.18	−.53**

Source: Compiled by the author from Thomas Mackie and Richard Rose, *International Almanac of Electoral History* (New York: Free Press, 1974); *Keesing's Archives;* Charles Taylor and Michael Hudson, *World Handbook of Political and Social Indicators* (New Haven, Conn.: Yale University Press, 1972); and Appendix 1.

Note: The control variables in the turnout equation are compulsory voting, population size and GNP/capita. The control variables in the deaths equations are ethnic fractionalization, population size, and GNP/capita.

* = F over 1.7 (significant at .1 in a sample).

** = F over 3.0 (significant at .05 in a sample).

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Post-Materialism in an Environment of Insecurity

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This article tests the hypothesis that postwar affluence led to an intergenerational shift from Materialist to Post-Materialist values among Western publics, and analyzes the consequences of the economic uncertainty prevailing since 1973. The young emphasize Post-Materialist values more than the old. Time-series data indicate that this reflects generational change far more than aging effects, but that the recession of the mid-1970s also produced significant period effects. As Post-Materialists aged, they moved out of the student ghetto and became a predominant influence among young technocrats, contributing to the rise of a "New Class." They furnish the ideologues and core support for the environmental, zero-growth and antinuclear movements; and their opposition to those who give top priority to reindustrialization and rearmament constitutes a distinctive and persisting dimension of political cleavage.

A decade ago it was hypothesized that the basic value priorities of Western publics had been shifting from a Materialist emphasis toward a Post-Materialist one—from giving top priority to physical sustenance and safety, toward heavier emphasis on belonging, self-expression and the quality of life. This shift was traced to the unprecedented levels of economic and physical security that prevailed during the postwar era (Inglehart, 1971). Since this first exploration, the Materialist/Post-Materialist value change hypothesis has been subjected to further analysis by dozens of investigators using field work carried out in the United States, Canada, Australia, Japan and 15 West European nations.[1] Measurements at multiple time points are now available for a number of these countries; in all, well over 100 representative national surveys have measured the prevalence of Materialist/Post-Materialist value priorities among the publics of advanced industrial societies. A disproportionate share of this research has taken place in Germany and Japan—two countries that have experienced rapid economic growth in recent decades, and relatively rapid value change. Less evidence has been gathered in the relatively stagnant United States, despite the dominant position this country has held until recently in empirical social research.

Our data now span a decade. Implications for political change that were suggested by the original cross-sectional analysis can be tested in diachronic perspective. We can begin to distinguish between: (1) intergenerational value change, based on cohort effects; (2) life cycle or aging effects; and (3) period effects; in particular, we can ask: have the economic uncertainty and the deterioration of East-West detente in recent years produced a sharp decline in Post-Materialism? As we will see, the answer is No. Overall there was remarkably little change in the ratio of Materialists to Post-Materialists among Western publics. But, like Sherlock Holmes's dog that did not bark in the night, this lack of dramatic change has crucial implications. Much of the literature on Post-Materialism deals with whether it is a deep-rooted phenomenon having a long-term impact on political behavior, or simply a transient epiphenomenon. We will reexamine this issue in the light of recent evidence. If a society's basic values change mainly through intergenerational population replacement, we would expect them to change at a glacial pace. But though short-term changes may be small, close examination of their societal location can provide valuable insight into their long-term implications. Contrary to what some observers have assumed (Kesselman, 1979), Post-

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[1] For representative examples, see Ike, 1973; Kerr and Handley, 1974; Marsh, 1975, 1977; Kmieciak, 1976; Lafferty, 1976; Hildebrandt and Dalton, 1977; Zetterberg, 1977; Watanuki, 1979; Inglehart, 1977; Jennings, Allerbeck and Rosenmayr, 1979; Lehner, 1979; Kaase and Klingemann, 1979; Heunks, 1979; Pesonen and Sankiaho, 1979; Kemp, 1979; Flanagan, 1979, 1980; Nardi, 1980.

Materialism has not dwindled away in the face of diminished economic and physical security. In most countries its numbers grew, and in some ways its political influence seems greater now than a decade ago; but its character and tactics have changed significantly.

One of the most important changes derives from the simple fact that today, Post-Materialists are older than they were when they first emerged as a major political factor in the 1960s. Initially manifested mainly through student protest movements, their key impact is now made through the activities of young elites. For the students have grown older, and Post-Materialism has penetrated deeply into the ranks of young professionals, civil servants, managers, and politicians. It seems to be a major factor in the rise of a "new class" in Western society—a stratum of highly educated and well-paid young technocrats who take an adversary stance toward their society (Ladd, 1978; Gouldner, 1979; Lipset, 1979; Steinfels, 1979). The current debate between those giving top priority to reindustrialization and rearmament, versus those who emphasize environmentalism and the quality of life, will not be easy to resolve: it reflects persisting value cleavages.

Reexamining the Theory of Value Change

Before turning to time series evidence, let us reexamine our theoretical framework in the light of recent findings. It is based on two key hypotheses:

1. *A Scarcity Hypothesis.* An individual's priorities reflect the socioeconomic environment: one places the greatest subjective value on those things that are in relatively short supply.
2. *A Socialization Hypothesis.* The relationship between socioeconomic environment and value priorities is not one of immediate adjustment: a substantial time lag is involved, for, to a large extent, one's basic values reflect the conditions that prevailed during one's preadult years.

The *scarcity hypothesis* is similar to the principle of diminishing marginal utility, in economic theory. A complementary concept—Abraham Maslow's (1954) theory of a need hierarchy underlying human motivation—helped shape the survey items we used to measure value priorities. In its simplest form, the idea of a need hierarchy would probably command almost universal assent. The fact that unmet physiological needs take priority over social, intellectual or esthetic needs has been demonstrated all too often in human history: starving people will go to almost any lengths to obtain food. The rank ordering of human needs becomes less clear as we move beyond those needs directly related to survival. But it *does* seem clear that there is a basic distinction between the "material" needs for physiological sustenance and safety, and nonphysiological needs such as those for esteem, self-expression and esthetic satisfaction.

The recent economic history of advanced industrial societies has significant implications in the light of the scarcity hypothesis. For these societies are a remarkable exception to the prevailing historical pattern: the bulk of their population does *not* live under conditions of hunger and economic insecurity. This fact seems to have led to a gradual shift in which needs for belonging, esteem and intellectual and esthetic satisfaction became more prominent. As a rule, we would expect prolonged periods of high prosperity to encourage the spread of Post-Materialist values; economic decline would have the opposite effect.

But it is not quite that simple: there is no one-to-one relationship between economic level and the prevalence of Post-Materialist values, for these values reflect one's *subjective* sense of security, not one's economic level *per se*. While rich individuals and nationalities, no doubt, tend to feel more secure than poor ones, these feelings are also influenced by the cultural setting and social welfare institutions in which one is raised. Thus, the scarcity hypothesis alone does not generate adequate predictions about the process of value change. It must be interpreted in connection with the *socialization hypothesis*.

One of the most pervasive concepts in social science is the notion of a basic human personality structure that tends to crystallize by the time an individual reaches adulthood, with relatively little change thereafter. This concept permeates the literature from Plato through Freud and extends to the findings of contemporary survey research. Early socialization seems to carry greater weight than later socialization.

This, of course, doesn't imply that no change whatever occurs during adult years. In some individual cases, dramatic behavioral shifts are known to occur, and the process of human development never comes to a complete stop (Levinson, 1979; Brim and Kagan, 1980). Nevertheless, human development seems to be far more rapid during preadult years than afterward, and the great bulk of the evidence points to the conclusion that the statistical likelihood of basic personality change declines sharply after one reaches adulthood. Longitudinal research following given individuals over periods as long as 35 years, shows strong correlations (as high as .70) between people's scores on standardized personality scales from young adulthood to middle age, or even old age (Block, 1981; Costa and McCrae, 1980).

Taken together, these two hypotheses generate a coherent set of predictions concerning value change. First, while the scarcity hypothesis implies that prosperity is conducive to the spread of Post-Materialist values, the socialization hypothesis implies that neither an individual's values nor those of a society as a whole are likely to change overnight. Instead, fundamental value change takes place gradually, almost invisibly; in large part, it occurs as a younger generation replaces an older one in the adult population of a society.

Consequently, after a period of sharply rising economic and physical security, one would expect to find substantial differences between the value priorities of older and younger groups: they would have been shaped by different experiences in their formative years. But there would be a sizeable time lag between economic changes and their political effects. Ten or fifteen years after an era of prosperity began, the age cohorts that had spent their formative years in prosperity would begin to enter the electorate. Ten more years might pass before these groups began to occupy positions of power and influence in their society; perhaps another decade would pass before they reached the level of top decision makers.

The socialization hypothesis complements the scarcity hypothesis, resolving objections derived from an oversimplified view of how scarcity affects behavior. It helps account for apparently deviant behavior: on one hand, the miser who experienced poverty in early years and relentlessly continues piling up wealth long after attaining material security, and on the other hand, the saintly ascetic who remains true to the higher-order goals instilled by his culture, even in the face of severe deprivation. In both instances, an explanation for the seemingly deviant behavior of such individuals lies in their early socialization.

The socialization hypothesis also explains why certain experimental tests of the need hierarchy have found no positive correlation between satisfaction of a given need at one time, and increased emphasis on the next higher need at a later time (Alderfer, 1972; Kmieciak, 1976). For these experiments are based on the implicit assumption that one would find almost *immediate* changes in an individual's priorities. But if, as hypothesized, an individual's basic priorities are largely fixed by the time he or she reaches adulthood, one would not expect to find much short-term change of the kind that was tested for.

This does not mean that an adult's value priorities are totally immutable—merely that they are relatively difficult to change. Normally, the rewards and deprivations employed in experimental psychology are modest, and the treatment is continued for a fairly brief time. Only in unusual experiments has the treatment been extreme enough to produce evidence of changed priorities among adults. In one such experiment, for example, a conscientious objector was kept on a semi-starvation diet for a prolonged period under medical supervision. After several weeks, he lost interest in his social ideals and began to talk about, *think* about and even *dream* about food (Davies, 1967). Similar patterns of behavior have been observed among inmates of concentration camps (Elkins, 1959; Bettelheim, 1979).

Marsh (1975) finds that Post-Materialists do not express higher satisfaction with their incomes than do Materialists. This is illogical, he argues: presumably, the former are Post-Materialists *because* their material needs are satisfied—so why don't they express relatively high levels of subjective satisfaction with their material circumstances? Once again the confusion is based on the implicit assumption that value change reflects an *immediate* response to one's environment. In the short run, one normally *does* experience a subjective sense of satisfaction when one satisfies material needs. But if these needs have been satisfied throughout one's formative years, one takes them for granted and develops higher expectations. In the long run, the fact that one has enough oxygen, water, food and clothing does *not* produce a subjective sense of satisfaction—which is precisely why the Post-Materialists seek satisfaction in *other* realms.

Because their incomes are higher than those of the Materialists, and yet they are still dissatisfied, Marsh concludes that Post-Materialists are actually *more* acquisitive than Materialists. Their emphasis on nonmaterial societal goals reflects mere lip service to fashionable causes, he argues, not their true personal values. Subsequent findings by Marsh himself refute this interpretation. To test his hypothesis, he developed an index of "Personal Post-Materialism"; he finds a correlation of +.22 between it and my index of societal Post-Materialism (Marsh, 1977, p. 180). While his discussion emphasizes the fact that this correlation is "only" .22, the crucial point is that the correlation is *positive*—and not negative, as he argued earlier. When one is dealing with survey data, a product-moment correlation is +.22 is fairly strong, particularly when it is found between two sets of items that were designed with the expectation that they would show a *negative* correlation.

Time Series Evidence from the Postwar Era

Our hypotheses imply that the unpredecented prosperity prevailing from the late 1940s until the early 1970s, led to substantial growth in the proportion of Post-Materialists among the publics of advanced industrial societies. We would need a time machine in order to go back and test this proposition, using the battery specifically developed to measure Materialist/Post-Materialist values. Though this is impossible, some available data *do* seem to tap the relevant dimension.

Data on the priorities of the German public, for example, cover more than 20 years, from 1949 to 1970. In these surveys, representative national samples were asked, "Which of the four Freedoms do you personally consider most important —Freedom of Speech, Freedom of Worship, Freedom from Fear or Freedom from Want?" In 1949, postwar reconstruction had just begun, and "Freedom from Want" was the leading choice by a wide margin. But in the following years, Germany rose from poverty to prosperity with almost incredible speed. In 1954, "Freedom from Want" was still narrowly ahead of any other choice, but by 1958 "Freedom of Speech" was chosen by more people than all other choices combined (EMNID, 1963, 1970).

These changes in the German population's value priorities seem to reflect the concurrent changes in their economic environment. And there is clear evidence of an age-related lag between economic change and value change. In 1962, 59 percent of the Germans from 16 to 25 years old chose "Freedom of Speech"; the figure declines steadily as we move to older groups; among Germans aged 65 and older, only 35 percent chose "Freedom of Speech." The fact that the young are much likelier to give "Freedom of Speech" priority over "Freedom from Want" fits theoretical expectations neatly. The original data have been lost, and it is not possible to perform an age cohort analysis in order to determine how much of this age difference is due to generational change. But the magnitude of the overall shift is so great that each age group must have *deemphasized* "Freedom from Want" as it aged during this period: the age differences *cannot* be attributed to life cycle effects. Further persuasive evidence of an intergenerational shift toward Post-Materialist priorities among the German public is found in the massive and definitive analysis of German survey data from 1953 through 1976 by Baker, Dalton and Hildebrandt (1981).

The most dramatic example of economic change in modern history is Japan—a nation that rose from harsh poverty to astonishing prosperity in a single generation. Indicators of the Japanese public's values are available in the Japanese national character studies carried out at five-year intervals, from 1953 through 1978. Analysis of these surveys indicates that Japanese culture changed along *several* different dimensions during this period, with the perceived sacredness of the emperor declining and emphasis on individuation and political participation rising (Ike, 1973; Hayashi, 1974; Nisihira, 1974; Richardson, 1974; Research Committee on Japanese National Character, 1979; Flanagan, 1979; Inglehart, 1982). One of the changes, it seems clear, was a shift from Materialist to Post-Materialist priorities. Among the available survey questions, the most unambiguous indicator of Materialist versus Post-Materialist priorities is the following: "In bringing up children of primary school age, some think that one should teach them that money is the most important thing. Do you agree or disagree?" In 1953, a strong majority (65 percent) of the Japanese public agreed that financial security was the most important thing. This figure declined steadily in subsequent surveys: by 1978 only 45 percent of the public still took this view. As was true of Germany, the trend is in the predicted direction—but in this case, the original data have been preserved and we can carry out a cohort analysis. Table 1 shows the results.

In any given year, the young are a good deal less likely to emphasize the importance of money than are the old. Does this simply reflect an inherent idealism of youth that will disappear as they grow older? Apparently not—for when we follow given age cohorts as they age during this 25-year period, we find no indication whatever of increasing materialism. Quite the contrary, we find a tendency for a given cohort as it grows older to place *less* emphasis on money: the five cohorts for which we have data throughout the 25-year period show an average shift of six points *away* from giving top priority to money. Almost certainly this was a period effect, with the sharply rising prosperity of the postwar era producing a diminishing emphasis on money within each age cohort, quite independently of generational change or aging effects. As closer examination of Table 1 indicates, this period effect operated rather strongly from 1953 to 1973 and then reversed direction, so that from 1973 to 1978 each age cohort came to place slightly *more* emphasis on the importance of money. This pattern reflects changes in the economic environment rather faithfully: the extraordinary rise in prosperity that took place in Japan from 1953 to 1973 was mirrored in a gradual deemphasis on money within each age cohort; and the economic uncertainty that followed the oil shock of 1973 was accompanied by a partial reversal of this trend.

But these period effects are dwarfed by the intergenerational differences. While period effects seem to account for a mean net shift of 6 percentage points away from emphasizing the importance of financial security, we find a difference of 44 points between the youngest and oldest groups in 1978. Since these data show no evidence whatever that aging leads to increasing emphasis on money, there is a strong *prime facie* case for attributing this 44-point difference entirely to inter-

Table 1. Cohort Analysis: Percentage of Japanese Public Agreeing That Financial Security Is Most Important

Age Group	1953	1958	1963	1968	1973	1978	Change Within Given Cohort 1953-1978	
20-24	60	–	43	34	22	18		
25-29	66		55	49	36	26		
30-34	63		58	58	42	37		
35-39	62		56	59	43	43		
40-44	65		63	59	46	49		
45-49	66		62	62	46	56	→ – 4	
50-54	72	–	68	65	49	51	→ –15	
55-59	72	–	72	67	60	56	→ – 7	Mean: –6
60-64	77	–	76	66	59	62	→ 0	
65-69	78	–	72	73	59	62	→ – 3	
Spread between Youngest and Oldest:	+18	–	+29	+39	+37	+44		

Source: Japanese National Character Surveys carried out by the Institute of Statistical Mathematics, Tokyo.

generational change. It is conceivable that a life cycle tendency toward increasing Materialism with increasing age *also* exists, but is totally concealed by stronger period effects working in the opposite direction: the complexities of distinguishing between aging effects, cohort effects and period effects are such that we can not totally exclude this possibility (Glenn, 1976; Knoke and Hout, 1976). But belief in such an aging effect must depend on faith alone; it is totally unsupported by empirical evidence.

Indications of intergenerational change, conversely, seem incontrovertible. In 1953, even the *youngest* group showed overwhelmingly Materialistic priorities—because at that time, *all* adult age cohorts had spent their formative years during World War II or earlier. These cohorts show only modest changes as they age during the ensuing quarter-century. It is only from 1963 on—when the postwar cohorts begin to enter the adult population—that we find a clear rejection of financial security as a value having top priority among the younger cohorts. The shift of the Japanese public from a heavy majority giving money top priority, to a minority doing so, seems to reflect intergenerational population replacement above all, with only a minor component due to period effects. By 1978, there was a tremendous difference between the priorities of younger and older Japanese. As the leading example of economic growth in the postwar era, Japan constitutes a crucial case for testing our hypotheses. The time series data are unambiguous: from 1953 to 1978 there was an intergenerational shift away from Materialism among the Japanese public.

Materialist and Post-Materialist Values from 1970 to 1979

Our data from Western countries cover a shorter period than those from Japan, but they were specifically designed to measure Materialist/Post-Materialist value priorities. It is difficult to measure values directly. But their presence can be inferred from a consistent pattern of emphasis on given types of goals. Accordingly, we asked representative samples of citizens from Western nations what they personally considered the most important goals among the following:

A. Maintain order in the nation
B. Give people more say in the decisions of the government
C. Fight rising prices
D. Protect freedom of speech
E. Maintain a high rate of economic growth
F. Make sure that this country has strong defense forces
G. Give people more say in how things are decided at work and in their community
H. Try to make our cities and countryside more beautiful
I. Maintain a stable economy
J. Fight against crime
K. Move toward a friendlier, less impersonal society
L. Move toward a society where ideas count more than money.

Our earliest survey (in 1970) used only the first four items, in six countries. The full 12-item battery was first used in 1973 in the nine-nation European Community and the United States (Inglehart, 1977). Both batteries were administered in numerous subsequent surveys. Items A, C, E, F, I and J were designed to tap emphasis on Materialist goals; theoretically, these values should be given high priority by those who experienced economic or physical insecurity during their formative years. The remaining items were designed to tap Post-Materialist goals; they should be emphasized by those raised under relatively secure conditions. If so, certain respondents would favor Materialist items consistently, while others would consistently emphasize the Post-Materialist ones.

Survey results support these theoretical expectations. Those who give top priority to one Materialist goal tend to give high priority to other Materialist goals as well. Conversely, the Post-Materialist items tend to be chosen together. Hence, we can classify our respondents as pure Materialists (those whose top priorities are given to Materialist goals exclusively); pure Post-Materialists (those whose top priorities are given to Post-Materialist items exclusively); or mixed types based on any combination of the two kinds of items. Though for simplicity of presentation we will usually compare the two polar types, we are dealing with a continuum having numerous intermediate categories.

The predicted relationships with social background are also confirmed empirically. Within any given age group, those raised in relatively prosperous families are most likely to emphasize Post-Materialist items, and the predicted skew by age group is manifest. Figure 1 depicts this pattern in the pooled sample of six West European publics interviewed in our initial survey. Significant cross-national differences exist, but the basic pattern is similar from nation to nation: Among the oldest group, Materialists outnumber Post-Materialists enormously; as we move toward younger groups, the proportion of Materialists declines and that of Post-Materialists increases.

A major watershed divides the postwar generation (in 1970, those 15 to 24 years old) from all other age groups. While Materialists are still more than twice as numerous as Post-Materialists among those 25-34 years old, when we move across the World War II watershed, the balance shifts dramatically, with Post-Materialists becoming more numerous than Materialists.

The Materialist and Post-Materialist types have strikingly different opinions on a wide variety of issues, ranging from women's rights, to attitudes toward poverty, ideas of what is important in a job, and positions on foreign policy. Within each age group, about half the sample falls into the mixed value types. On virtually every issue, their position is between the Materialists and Post-Materialists: they seem to be a cross-pressured group that could swing either way.

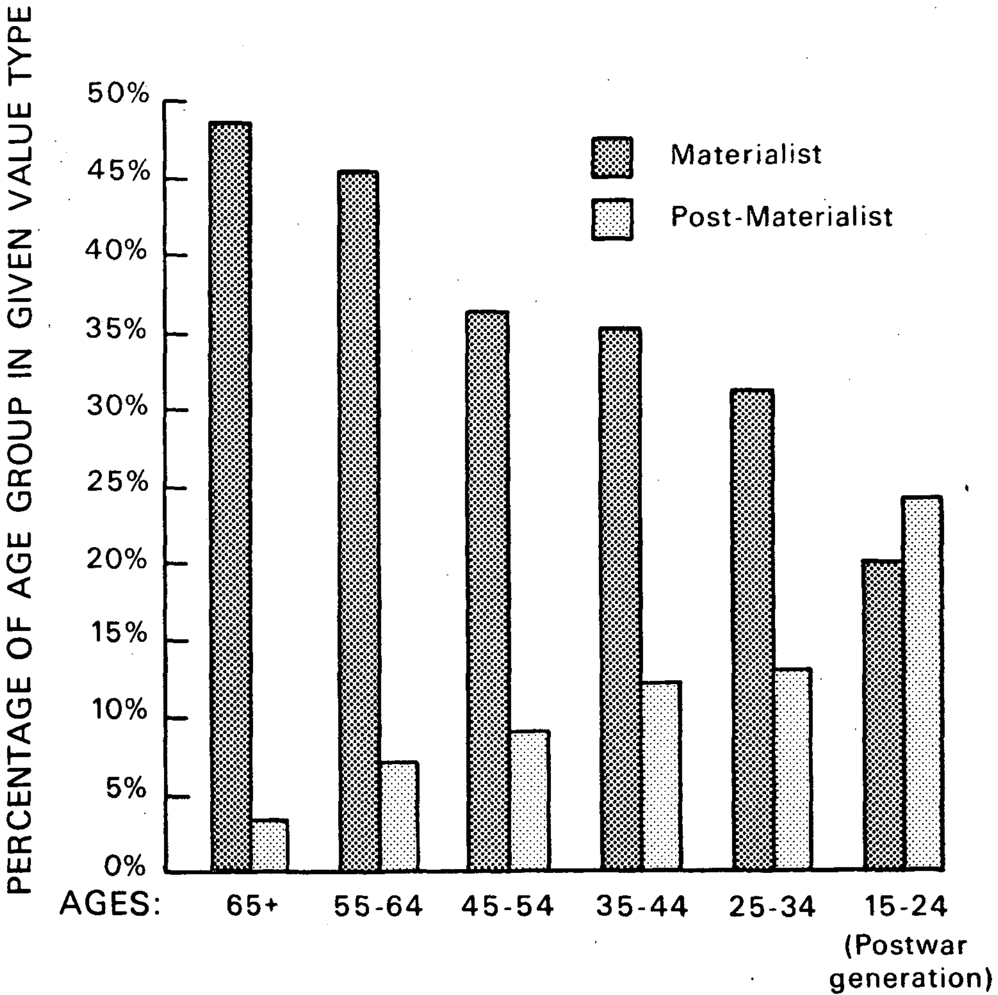
By 1970, Post-Materialists had attained numerical parity with Materialists *only* among the postwar generation. Furthermore, they were concentrated among the more affluent strata of this age group: among university students, they heavily outnumbered the Materialists. In this light, perceptions of a generation gap in the late 1960s and early 1970s are understandable. Even among the postwar generation, Materialists were about as numerous as Post-Materialists. But in this age group's most articulate and most visible segment—the students—there was an overwhelming preponderance of Post-Materialists. The students lived in a distinct milieu: they had highly developed communications networks with other students but were largely isolated from their nonstudent peers. The priorities prevailing in this milieu were fundamentally different from those shaping the society as a whole.

The existence of such a milieu can play an important part in the evolution and propagation of a given set of values. Indeed, Habermas (1979) argues that the rise of Post-Materialism is not due to the different formative experiences of different generation units, but to exposure to the specific world views inculcated by distinct communications networks (c.f. Jaeggi, 1979). This explanation seems to complement, rather than substitute for, the one proposed here. It helps account for the spread of values in a given milieu, but provides no explanation why given generation units were disposed to accept given values in the first place, while others rejected them. Nevertheless, it seems clear that in virtually all Western nations, the student milieu of the late 1960s *did* constitute a distinct communications network, propagating a distinctive viewpoint. Given these circumstances, it is not surprising that the student elite saw themselves as part of a counterculture that was engaged in an irreconcilable clash with the culture of an older generation: From their viewpoint, the dictum, "Don't trust anyone over thirty" seemed plausible. Our hypotheses imply that as time went by, the Post-Materialists became older and more evenly distributed across the population. But in 1970, conditions were optimal to sustain belief in a monolithic generation gap, with youth all on one side and older people all on the other.

Clearly, there are large empirical differences between the priorities of younger and older groups in Western Europe (and, as subsequent research revealed, the entire Western world). But one can advance various interpretations concerning the *implications* of this finding. Though our own hypotheses point to intergenerational change based on cohort effects, we must acknowledge that any given pattern of age differences could, theoretically, result from (1) aging effects, (2) cohort effects, (3) period effects, or some combination of all three.

1. Aging Effects versus Cohort Effects. Perhaps the most obvious alternative interpretation is one based on aging effects. It would argue that for biological or other reasons, the young are inherently less materialistic than the old. As they age, however, they inevitably become just as materialistic as their elders; after 50 years, the youngest group will show the same overwhelming preponderance of Materialists that the oldest group now displays. The aging interpretation, then, holds that the pattern found in 1970 is a permanent characteristic of the human life cycle and will not change over time. The cohort interpretation, on the other hand, implies that the Post-Materialists will gradually permeate the older strata, neutralizing the relationship between values and age.

The most dramatic change from one age group to the next, in Figure 1, is the sudden shift in the balance between Materialist and Post-Materialist types that occurs as we move from the second-youngest to the youngest group. Our hypotheses imply the existence of a significant watershed between the postwar generation and the older groups that had experienced the World Wars, the



Source: European Community survey carried out in Feb.-Mar. 1970, sponsored by Commission of the European Communities; principal investigators were Jacques-René Rabier and Ronald Inglehart.

Figure 1. Value Type by Age Group, among the Publics of Britain, France, West Germany, Italy, Belgium, and The Netherlands in 1970

Great Depression and their associated threats to economic and physical security. The gap between the two youngest groups in 1970 fits the historical change theory neatly. But this gap could *also* be interpreted as a permanent feature of the human life cycle. For in 1970, the dividing line between the postwar generation and all older groups happened to coincide with the boundary between those 25 years of age or older, and those who were 24 or younger. Since we know that people tend to get married, have their first child, and begin a permanent career at about this age, it might be argued that what we have identified as an historical watershed between the postwar generation and the older ones, merely reflects the stage in the life cycle when people get married and settle down. Time series data are needed to determine which interpretation is correct.

2. Period Effects. Both the German data and the Japanese data reviewed earlier show period effects: the economic environment of the period up to 1973 apparently induced *all* age groups to become less materialistic as time went by, quite apart from any processes of aging or generational change. These surveys were executed during a period of dramatic improvement in living standards, particularly in Germany and Japan. But even in the United States (where economic growth was much slower) the real income of the American public approximately doubled from 1947 to 1973.

From 1973 on, however, economic conditions changed drastically. Energy prices quadrupled almost overnight; the industrialized world entered the most severe recession since the 1930s. Economic growth stagnated and Western nations experienced extraordinarily high levels of inflation *and* unemployment. By 1980, the real income of the typical American family was actually *lower* than in 1970.

Western publics were, of course, acutely aware of changed economic circumstances, and responded to them. The most amply documented case is that of the American public, whose economic outlook is surveyed each month. In mid-1972, the University of Michigan Survey Research Center's Index of Consumer Sentiment stood at 95, only slightly below its all-time high. By the spring of 1975, the SRC Index had plummeted to 58—the lowest level recorded since these surveys were initiated in the 1950s. With the subsequent economic recovery, consumer confidence revived—only to collapse again in the wake of the second OPEC price shock in late 1979; in April, 1980 consumer confidence had reached a new all-time low, with the SRC Index at 53. Similar patterns of declining confidence in the economic outlook were recorded among West European publics (Commission of the European Communities, 1979).

The sense of physical security has also declined. The Soviet arms buildup, their invasion of Afghanistan, and the Western response to these events led to an erosion of East-West detente. This, too, had a pronounced impact on the outlook of Western publics. In the fall of 1977, the publics of the European Community nations were asked to assess the chances of a world war breaking out within the next ten years. Only 26 percent of the nine publics (weighted according to population) rated the likelihood at 50 percent or greater. This was roughly comparable to the results obtained when a similar question was asked in July, 1971. In April, 1980, however, fully 49 percent of the nine publics rated the danger at 50 percent or greater: such pessimism had almost doubled since 1977 (Euro-Barometer 13, 1980, p. 16).

Clearly (as the scarcity hypothesis implies), the period effects of recent years should inhibit the development of a Post-Materialist outlook. And the socialization hypothesis implies that current conditions would have their greatest impact on the youngest and theoretically most malleable respondents—those aged 15-24, who are still in their formative years.

Which of these three processes was most important during the 1970s? Given the severity of the economic decline and the almost total disappearance of student protest and other dramatic manifestations of a counterculture, one might assume that Post-Materialism has been swept away completely by a new, harsher environment. Or—as the socialization hypothesis suggests—are these priorities sufficiently deep-rooted among the adult population to weather the effects of the current socioeconomic environment?

Table 2 provides part of the answer. It shows the distribution of the two polar value types from early 1970 to late 1979, in the six countries for which we have data covering this entire time span, and in the United States from 1972 to 1980. Of necessity, we use the original 4-item index here. By contrast with the cataclysmic changes that took place in consumer confidence indices and in perceptions of the danger of war, the changes here are remarkably small: for the most part, the shifts from year to year fall within the range of normal sampling error. Moreover, only modest and nonlinear cumulative changes took place from 1970 to 1980, and they vary cross-nationally. In four of the seven countries—Great Britain, Germany, France and The Netherlands—there were fewer Materialists and more Post-Materialists at the end of the decade than at its start. In two countries (Belgium and the United States) there was virtually no change. Only in the seventh country—

Table 2. Changes in Prevalence of Materialist and Post-Materialist Value Types, 1970-1980 (Percentage falling into the two polar types)

	1970	1973	1976	1979
Britain				
Materialist	36	32	36	27
Post-Materialist	8	8	8	11
Germany				
Materialist	43	42	41	37
Post-Materialist	10	8	11	11
France				
Materialist	38	35	41	36
Post-Materialist	11	12	12	15
Italy				
Materialist	35	40	41	47
Post-Materalist	13	9	11	10
Belgium				
Materialist	32	25	30	33
Post-Materialist	14	14	14	14
Netherlands				
Materialist	30	31	32	28
Post-Materialist	17	13	14	19

	1972	1976	1980
United States			
Materialist	35	31	35
Post-Materialist	10	10	10

Source: European Community surveys carried out in Feb.-Mar. 1970; Sept. 1973; Nov. 1976; and Nov. 1979; and Post-Election wave of the U.S. National Election surveys carried out in each respective year by the Center for Political Studies, Institute for Social Research, University of Michigan.

Italy—do we find a shift toward the Materialist pole.

The fact that Italy is the deviant case is not surprising. During the 1970s Italy not only experienced exceptionally severe economic difficulties, but also severe political disorder. Probably for this reason, there was a substantial net shift toward Materialism among the Italian public. But in the other six countries, the process of population replacement outweighed the effects of economic and physical insecurity. The net result for the seven countries as a whole is that Post-Materialists were slightly *more* numerous at the end of the 1970s than they were at the start.

The impression of remarkable stability that the aggregate national data convey, conceals an extremely interesting underlying pattern. For, as hypothesized, the distribution of values across age groups has changed over time. Table 3 shows this relationship, from early 1970 through late 1979. Figure 2 depicts these data in graphic form. As these data reveal, the overall stability shown in Table 2 is the result of two opposing processes that largely cancel each other.

On one hand, the youngest group shows a substantial *decline* in the ratio of Post-Materialists to Materialists. In 1970, Post-Materialists were 4 percentage points more numerous than Materialists among this group: in other words, the group showed a Percentage Difference Index (PDI) of +4. This fell to a PDI of —1 in 1973 and fell further to a PDI of —5 in 1976. Although the trend then reversed itself, with a partial recovery to an index of —3 in 1979, this youngest group showed a net shift of 7 points in the Materialist direction during the 1970s.

Table 3. Changes in Prevalence of Materialist and Post-Materialist Values, 1970-1979 by Age Group (Percent) (Combined results from six European nations)

	1970		1973		1976		1979	
Ages	M	P-M	M	P-M	M	P-M	M	P-M
15-24	20	24	21	20	25	20	24	21
25-34	31	13	28	13	29	16	27	17
35-44	35	12	35	9	35	11	33	13
45-54	36	9	39	7	39	8	41	10
55-64	45	7	43	6	47	6	41	8
65 and over	48	3	45	4	52	5	49	5
Total:	35	12	34	10	37	12	35	13
Percent Difference Index:	−23		−24		−25		−22	

Source: Surveys sponsored by the Commission of the European Communities, carried out in Feb.-March, 1970; Sept. 1973; Nov. 1976 and Nov. 1979.

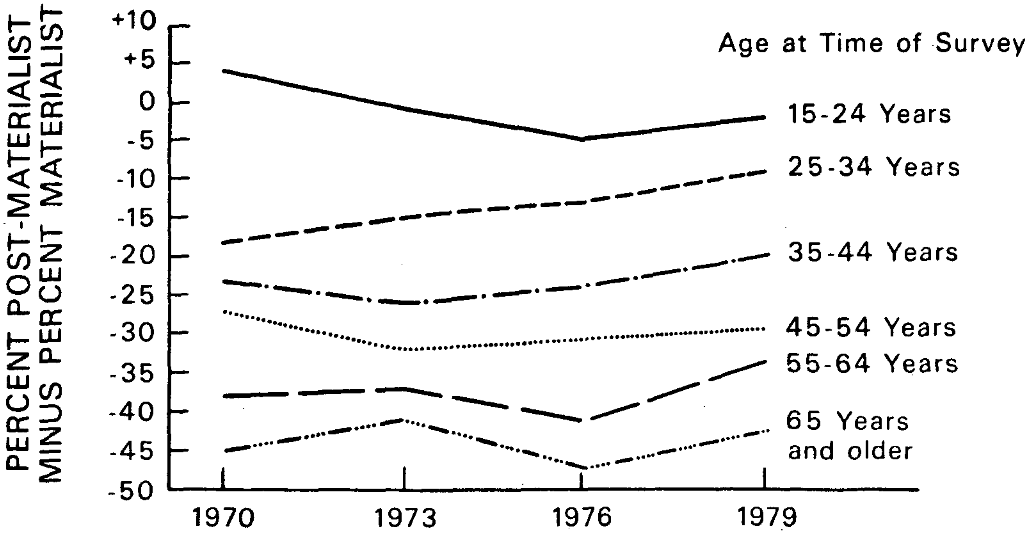
But this shift was more than offset by shifts in the *opposite* direction among the older groups. The socialization hypothesis implies that period effects would have their greatest impact on the youngest group. Empirically, it turns out that the youngest group was the *only* group on which period effects had a significant negative net impact: the older ones moved in the Post-Materialist direction. The American data show a similar pattern: younger respondents became more Materialist but the older ones became *less* so. In Europe, this countervailing tendency was especially strong among the second youngest group, which showed a steady rise in the proportion of Post-Materialists even during the depths of the 1970s recession; despite economic uncertainty and the erosion of detente, by 1979 this group had registered a net shift of 8 points toward the Post-Materialist pole.

This seemingly counterintuitive development reflects changes in the composition of the 25-34-year-old group, due to population replacement. In 1970 this group contained no one born after 1945; but by late 1979 its members were recruited entirely from the postwar generation. The World War II watershed now fell between the *two* youngest groups and all of the older ones.

Let us examine Figure 2 more closely. In 1970, by far the widest gap between adjacent age groups was that between the youngest and second-youngest groups. This gap was a prominent feature of the value distributions in each of the six nations surveyed in that year. At that point, it was unclear whether this gap should be interpreted as a World War II watershed, or as the reflection of a major life-cycle transition that takes place in one's mid-twenties.

That uncertainty has now vanished. During the 1970s, the still-malleable 15-24-year-old group became progressively more Materialist until the recession bottomed out, and then staged only a partial recovery. But the 25-34-year-old group became steadily more Post-Materialist throughout the 1970s. By the end of 1979 the largest gap no longer was located between these two youngest groups, but between the 25-34-year-old group, and those aged 35-44. This accords perfectly with the fact that members of the postwar generation had now reached 34 years of age.

It is virtually impossible to interpret this pattern in terms of aging effects—unless one believes that the human life cycle changed radically between 1970 and 1979: the crucial life stage transition that took place at age 25 in 1970, had somehow shifted to age 35 in 1979. Furthermore, there is no indication that aging has an inherent tendency to produce a Materialistic outlook. Despite the economic uncertainty of this period, only the 15-24-year-old group became significantly more Materialist; among older groups, the downward pressure of period effects was more than offset by



Source: European Community surveys carried out in Feb.-Mar. 1970; Sept. 1973; Nov. 1976 and Nov. 1979 in Britain, Germany, France, Italy, Belgium and The Netherlands: since data from only these nations are available for 1970, only these data are used for the subsequent time points. Surveys were sponsored by the Commission of the European Communities; principal investigators were Jacques-René Rabier and Ronald Inglehart.

Figure 2. Change in Value Priorities in Six Nations, 1970-1979, by Age Group

the upward pressure of the population replacement process. Furthermore, not even the youngest category showed a *continuous* downward trend: plummeting steeply until 1976, it began to reverse itself with recovery from the recession. It seems far more plausible to attribute this nonlinear pattern to period effects than to the aging process (which presumably is continuous).

The economic and physical uncertainty of the 1970s produced a significant period effect. The net movement toward Post-Materialism that would be expected from population replacement slowed to a crawl. By the decade's end, the 15-24-year-old group was significantly less Post-Materialist than their counterparts a decade earlier had been. This is congruent with impressionistic observations that the student population no longer seems as nonconformist as it once was. For while young people have become more materialistic, those in their thirties and forties have become less so: the notion of a generation gap, already an oversimplification in 1970, was even farther from reality in 1979 due to this convergence.

Post-Materialism and Political Change

The evidence indicates that Post-Materialism is a deep-rooted phenomenon. Despite the recession of recent years, it not only persisted but increased its penetration of older groups. What are the political implications? The remainder of this article will focus on that question.

One would expect that, despite their favored socioeconomic status, Post-Materialists would be relatively supportive of social change, and would have a relatively high potential for unconventional and disruptive political action. The reasons can be summarized as follows: (1) Materialists tend to be preoccupied with satisfying immediate physiological needs; Post-Materialists feel relatively secure about them and have a greater amount of psychic energy to invest in more remote concerns such as politics. (2) As a recently emerging minority whose highest priorities have traditionally been given relatively little emphasis in industrial society, Post-Materialists tend to be relatively dissatisfied with the established order and relatively supportive of social change. (3) The disruption and property damage that sometimes result from unconventional political action seem less negative to Post-Materialists, since they threaten things they value less than Materialists do. In short, Post-Materialists have a larger amount of psychic energy available for politics, they are less supportive of the established social order, and, subjectively, they have less to lose from unconventional political action than Materialists. Consequently, the rise of Post-Materialism has made available a new, predominantly middle-class base of support for the left.

Is it empirically true that Post-Materialists tend to be relatively change-oriented and ready to engage in unconventional political protest? One measure of support for social change is now a standard feature of the European Community surveys carried out twice each year. Representative national samples of the publics of the European Community countries are asked:

> On this card are three basic kinds of attitudes toward the society we live in. Please choose the one which best describes your opinion. (1) The entire way our society is organized must be radically changed by revolutionary action. (2) Our society must be gradually improved by reforms. (3) Our present society must be valiantly defended against all subversive forces.

These alternatives might be described as Revolutionary, Reformist and Conservative.

The Reformists constitute a clear majority—63 percent of those responding—in the European Community as a whole, and this holds true for all value types. But there is a pronounced difference between Materialists' and Post-Materialists' attitudes toward social change. Table 4 shows the relationship between value type and attitudes toward social change among the publics of the European Community as a whole. Among Materialists, only 4 percent endorse the Revolutionary option; nearly ten times as many (38 percent) support the Conservative option. Among Post-Materialists, by contrast, the Revolutionary option draws *more* support than the Conservative one. For both, the Reformist option is the leading choice—but Post-Materialists are only about one-third as likely to hold Conservative views as are Materialists, and about four times as apt to favor radical social change.

Despite their relatively privileged social status, Post-Materialists are markedly more favorable to social change than are other value types. But how far are they ready to go on behalf of their values?

A central feature of an eight-nation survey carried out during 1974-1976 was a scale designed to measure an individual's potential for participation in political protest activities (for details of field work, see Barnes, Kaase et al., 1979). This scale is based on whether the respondent has done, or would do and approves of, a series of acts ranging from circulating petitions to occupying buildings and disrupting traffic. Tested and applied in eight countries, the scale has remarkably good technical characteristics and seems to measure, in a straightforward way, just how far an individual is willing to go in order to carry out his or her beliefs.

Do Post-Materialists show a relatively high pro-

Table 4. Support for Social Change versus Resistance to Social Change, by Value Type, 1976-1979 (percent)

Respondent's Value Type	Support for Revolutionary Change	Support for Gradual Reform	Support for Defense of Present Society	N
Materialist	4	57	38	18,292
Mixed	8	62	30	26,694
Post-Materialist	17	69	14	6,098

Source: Pooled data from Euro-barometre surveys, 1976 through 1979, from all nine European Community nations.

Note: Results are weighted according to size of each nation's population.

pensity for unconventional political activities as measured by the Protest Potential Scale? On one hand, Marsh (1975) has argued that Post-Materialist responses tap nothing more than a form of radical chic among a basically conservative elite. If so, the answer would be No. On the other hand, if our typology reflects basic value differences, the answer should be Yes. The Protest Potential Scale was developed, in part, to test whether Post-Materialist values go no deeper than lip service to fashionable goals, or whether they have behavioral implications. The answer is unequivocal.

Table 5 shows the relationship between protest potential and value type in each of the eight nations surveyed in 1974-1976. A score of "3" is the cutting point for this table: Those scoring below this level are (at most) ready to circulate petitions or to march in peaceful demonstrations; a good many of them have done nothing at all and are not willing to do anything. Those scoring higher are willing to do all of the above *and* engage in boycotts; many are ready to go still farther—to take part in rent strikes, illegal occupation of buildings or to block traffic.

As Table 5 makes clear, people's value priorities have a strong relationship to their level of Protest Potential. In Great Britain (where the relationship is weakest), only 21 percent of the Materialists are ready to engage in boycotts or go beyond them in protest against some form of perceived political injustice; among the Post-Materialists, 55 percent have done so, or are willing to do so. The linkage between values and the potential for protest is particularly strong in Italy, where only 20 percent of the Materialists rank high on protest potential—as compared with 69 percent of the Post-Materialists. In all eight countries Post-Materialists are far readier to engage in political protest than Materialists.

In multivariate analyses, when we control for the effects of age, education, income, and one's level of ideological sophistication, a strong relationship persists between Post-Materialist values and a predisposition for unconventional protest. A relatively high potential to use unconventional and disruptive techniques in order to intervene in the political process seems to be directly linked with the Post-Materialist outlook; it is not merely a spurious correlate, resulting from the fact that Post-Materialists tend to be young and well educated.

This might seem paradoxical. On one hand, we have seen that the economic uncertainty of the 1970s did not cause Post-Materialistm to disappear—on the contrary, its support seemms to have grown in most Western countries. On the other hand, the dramatic political protest move-

Table 5. Protest Potential, by Value Type in Eight Western Nations, 1974-1976 (Percent)

Respondent's Value Type	The Netherlands	Britain	United States	Germany	Austria	Italy	Switzerland	Finland
Materialist	27	21	38	23	17	20	17	20
Mixed	42	31	48	36	21	38	27	34
Post-Materialist	74	55	72	74	48	69	61	58

Source: Eight-Nation survey, carried out 1974-1976; for details of fieldwork and a report of findings from five of these eight nations, see Samuel H. Barnes, Max Kaase, et al., *Political Action* (Beverly Hills, Calif.: Sage, 1979).

Note: Figures represent percent scoring "3" or higher on Protest Potential scale. Values index is based on items A-L cited above.

ments of the late sixties and early seventies have disappeared in the United States (though not in Western Europe): why did this happen, if a relatively high protest potential characterizes the Post-Materialists?

The main answer is that people don't protest in a social vacuum. The Post-Materialists did not protest for the sake of protesting—they responded to specific issues, above all the war in Indochina. The fact that there no longer is a war in Indochina (or, at least, no *American* war) makes a big difference. Almost nothing can compare with war, in terms of violence, drama and human tragedy, and nothing on the current scene can command sustained mass attention in the way the Vietnam War did. In the absence of any political cause fully comparable to the war, it is only natural that much of the attention and energies of Post-Materialists have been diverted into other channels.

For some, this means seeking self-actualization through development of the inner self, rather than through social action. The human potential movement is an example. For those who remain politically active, this turning inward seems like desertion of the cause; the current crop of youth has been characterized as the Me Generation, practicing a culture of narcissism. There is some truth and even more misapprehension in this view. As we have seen, at the close of the 1970s the 15 to 24-year-old group *was* more materialistic than their counterparts at the start of the decade had been—but the difference was modest. If the potential for political protest generally remained only a potential, it may have been because none of the current political causes were as compelling as those of the earlier decade.

Post-Materialist Penetration of Elite Groups

But another major factor has also affected how Post-Materialism manifests itself politically. It springs from the simple fact that the average Post-Materialist is substantially older than he or she was in 1970. Post-Materialists are no longer concentrated in a student ghetto. They have moved into positions of influence and authority throughout society. Despite their minority status in society as a whole, they outnumber the Materialists in certain key sectors.

Table 6 shows how the two pure value types are distributed by age and occupation among the publics of the European Community countries. This table pools the data from six surveys carried out in all nine countries in the late 1970s, in order to provide reliable data for certain small but important elite groups. A national sample survey normally contains only a handful of professionals, for example; but here we have large enough numbers that we not only can compare this occupational category with others, but can break it down by age group.

The pure Post-Materialist type comprises only about 8 percent of the manual workers, and 7 percent of the farmers; in these occupational groups, pure Materialists outnumber Post-Materialists by ratios of at least five to one. On the other hand—despite the recent decline in Post-Materialism among the student-age population—Post-Materialists continue to outweigh Materialists in the student milieu. This is significant but not particularly surprising. What *is* surprising is the fact that among those less than 35 years old with jobs that lead to top management and top civil service posts, Post-Materialists outnumber Materialists decisively: their numerical preponderance here is even *greater* than it is among students. This is all the more astonishing since these young technocrats are older, on the average, than the students. This phenomenon reflects the fact that the young managers and officials are a highly select stratum, recruited according to considerably more demanding criteria than those for admission to a university: there are many more students than young technocrats. In social background, the latter correspond to the students at the most prestigious schools, rather than to the student population as a whole. In general, the more selective our criteria become, the higher the proportion of Post-Materialists—and the young technocrats represent the elite of the recent university graduates. Already occupying influential staff positions, many of them should reach the top decision-making level within the next decade.

Apart from the students, the young technocrats are the only category in which Post-Materialists hold a clear preponderance over Materialists. But Post-Materialism has also made impressive inroads in certain other elite categories. In the free professions, Post-Materialists are almost ass numerous as Materialists—not only in the under-35 age group but in the 35 to 49-year-old category as well. And there is an even balance between Materialists and Post-Materialists among those aged 35 to 49 in top management and top civil service posts.

Among those 50 and older, the Materialists hold a clear preponderance in *every* occupational category. Among self-employed people in business and trades in this age group, Materialists are six times as numerous as Post-Materialists. And even among the self-employed under 35, Materialists predominate by a ratio of nearly three to one: young technocrats may be Post-Materialistic, but young self-employed business people definitely are *not*.

By the end of the 1970s, Post-Materialism had become a powerful influence among technocrats and professionals in their thirties and forties. This does not mean that it will automatically become the dominant influence in Western societies. Post-Materialists remain a numerical minority, better equipped to attain their goals through bureaucratic institutions or the courts than through the electoral process. They may encounter the backlash of resurgent Materialism, as recent political events in the United States suggest. Nevertheless, by the late 1970s Post-Materialism had not only made deep inroads among young technocrats; but it had *also,* to a surprising degree, penetrated the West European political class.

Table 6 may actually *understate* the degree to which younger Western elites had become Post-Materialist. For these data are based on representative national samples; even when we break them down into relatively fine categories as in Table 6, even the top management and civil service group is fairly heterogeneous, ranging from relatively modest levels to the truly elite stratum. Theoretically, the latter should be more Post-Materialistic. For an indication of the values prevailing among West European political elites, let us turn to some data from interviews with the candidates running for seats in the European Parliament in 1979. This sample includes candidates from all significant political parties in all nine countries, drawn in proportion to the number of seats each party holds in the European Parliament (for details of fieldwork, see Inglehart, et al., 1979). In background, these candidates are roughly similar to members of the respective national parliaments (in which many of them hold seats). Though somewhat younger than the average member of the national parliaments, they seem to provide a reasonably good sampling of the West European political elite.

The candidates for the European Parliament were asked to rank their priorities among the 12-item set of societal goals described above. Those who gave top priority to one Materialist item tended to give high priority to the other Materialist items as well; the same was true of Post-Materialist items. Table 7 shows how the 12 items fell into two clearly defined clusters on the first dimension of a factor analysis. The structure of the candidates' choices shown here is virtually identical to that found among the publics of these same nine countries (compare the results in Inglehart, 1977, p. 46). Empirically, all six of the items designed to tap Materialist priorities show negative polarity, while all six items designed to tap Post-Materialist priorities show positive polarity.

The Materialist/Post-Materialist dimension proves to be remarkably robust—not only cross-culturally but also across time, and from the mass to the elite level. We used responses to these items to construct a value priorities index, coding those candidates who chose Materialist items for both their first and second priorities as pure "Materialists"; coding those who chose two Post-Materialist items as "Post-Materialist"; and those who chose other combinations as "Mixed." Table 8 shows the distribution of value types among our sample of the European Parliament, weighted in proportion to how many seats each party and nationality obtained in 1979.

In the Parliament as a whole, Materialists narrowly outnumber the Post-Materialists. But while Post-Materialists (as defined here) comprise little more than one-eighth of the general public of these countries, they make up nearly one-third of the European Parliament. And when we break our sample down by age group, we find a pronounced skew. Among those 55 years of age and

Table 6. Materialist versus Post-Materialist Values by Respondent's Occupation and Age Group in Nine European Community Nations, 1976-1979 (Percent)

	Age Less than 35			Ages 35-49			Age 50 and Over		
	M	P-M	N	M	P-M	N	M	P-M	N
Top management + top civil servants	20	30	565	22	22	702	28	16	374
Students	20	25	3,800	–	–	–	–	–	–
Professionals	25	21	280	21	19	218	29	12	162
Nonmanual employees	26	18	4,591	34	13	2,918	38	9	1,569
Unemployed persons	24	16	875	38	9	279	48	6	321
Self-employed business persons	35	13	1,329	41	10	1,109	43	7	855
Manual workers	32	11	4,673	40	8	3,264	44	5	2,255
Housewives	38	9	3,469	46	6	3,763	50	5	4,755
Farmers	42	10	347	44	5	528	48	4	778
Retired persons	–	–	–	–	–	–	51	5	7,018

Source: Based on combined data from the nine-nation European Community surveys carried out from 1976 through 1979.

Table 7. Value Priorities of Candidates to the European Parliament, 1979
(First factor in principal components factor analysis)

	Materialist/Post-Materialist (24%)	
Post-materialist goals	More say on job	.660
	Less impersonal society	.478
	More say in government	.472
	Society where ideas count	.408
	More beautiful cities	.315
	Freedom of expression	.254
Materialist goals	Control of inflation	–.436
	Fight against crime	–.442
	Stable economy	–.450
	Economic growth	–.566
	Maintain order	–.588
	Adequate defense forces	–.660

Source: Interviews with 742 candidates for seats in the European Parliament, carried out Mar.-Apr.-May, 1979 as part of the European Election Study organized by Karlheinz Reif. Principal investigators were Ian Gordon, Ronald Inglehart, Carsten Lehman Sorensen and Jacques-René Rabier.

over, Materialists are almost twice as numerous as Post-Materialists. But among those under 55, the Post-Materialists outnumber the Materialists.

Clearly, Post-Materialism is no longer a student phenomenon. When the postwar generation first became politically relevant in the 1960s, the universities may have been the only major institutions in which they were the dominant influence. Their youth, their minority status in society as a whole, and their relative lack of representation at decision-making levels dictated a protest strategy. Post-Materialists had little access to key decision-making posts; but they were highly motivated and articulate. They could not control decision making, but they *could* disrupt it—and they made use of unconventional political protest techniques.

Post-Materialism and the Rise of the New Class

The relative youth and powerlessness of the Post-Materialists may have dictated a strategy of student protest in the 1960s. But Post-Materialism has moved out of the student ghetto. By 1980, a Post-Materialist outlook had become more common than a Materialist one among young technocrats, professionals and politicians of Western countries. As experts, congressional staffers and members of ministerial cabinets, Post-Materialists had direct access to the command posts of the sociopolitical system. Protest was no longer their most effective tool. The impact of Post-Materialism was no longer symbolized by the student with a protest placard, but by the public interest law-

Table 8. Value Type by Age Group, among Candidates for European Parliament, 1979 (Percent)

	Ages		Total for European Parliament
	25-54	55-86	
Materialist	31	43	35
Mixed	34	34	34
Post-Materialist	36	23	32
N =	439	221	660

Source: Interviews with sample of candidates running for seats in European Parliament, interviewed in March-May, 1979. This sample includes 62 percent of those actually elected in June, 1979.

Note: Percentages are weighted in proportion to actual number of seats each party and nationality had in the European Parliament as of June, 1979. Accordingly, candidates from parties that won no seats are excluded from this table. Unweighted Ns appear at foot of each column. One column does not add up to 100 because of rounding.

yer, or the young technocrat with an environmental impact statement.

In recent years, a growing number of Western intellectuals have focused their attention on the rise of "The New Class." In contrast with the establishment-oriented New Class of Eastern Europe described by Djilas (1966), the New Class in the West is an elite characterized by its adversary stance toward the existing social order (Podhoretz, 1979; Bruce-Briggs, 1979); by its "culture of critical discourse" (Gouldner, 1979, pp. 28-29); and by a "new liberalism" (Ladd, 1978, pp. 48-49). Broder (1980) describes the emergence of a new generation of political elites have many of these characteristics as a "changing of the guard."

There is no clear consensus on the criteria that define the New Class. Ehrenreich and Ehrenreich (1977) describe it as those in the census categories of "professional and technical," plus "managers and administrators"—precisely the categories we have found to be most heavily Post-Materialist. But Ladd (1978) extends its limits to include anyone with a college education. There is even less consensus concerning *why* this well-paid and increasingly powerful stratum of society is critical of the existing economic and political order and participates in leftist political movements. There is a tendency to view an adversary culture as something *inherent* in higher education or in certain occupations, but the reasons are not altogether clear. Highly educated groups have existed for a long time, but in the past they generally were politically conservative. High levels of education and information are the *resource* that enables the New Class to play an important role—but they do not explain why today, an increasing share of the most highly educated and informed strata take an adversary stance toward their society.

I suggest that the rise of Post-Materialism and its subsequent penetration of technocratic and professional elites has been a major factor behind the emergence of the New Class. For this group is distinctive not only in its occupational and educational characteristics, but also in its values. And the ideology attributed to the New Class reflects Post-Materialist values rather closely (Ladd, 1976, 1978). If this is true, it explains why a New Class having these specific characteristics has emerged at this particular point in history.

For the distinctive values of the New Class reflect an historical change that can not be attributed simply to a changing educational and occupational structure. Rising levels of education and a shift of manpower into the "knowledge industries" have played a major role in the rise of this new elite, as Bell (1973, 1976), Lipset and Dobson (1972), Lipset (1979), and others have argued. But—as Table 6 makes clear—an "adversary culture" is not an *inherent* concomitant of the education or adult role of professionals and technocrats. *Older* professionals and technocrats are preponderantly Materialist; it is only among the younger segments of these groups that Post-Materialist priorities outweigh Materialist ones.

Because both the political environment and the social location of Post-Materialists have changed significantly, their tactics have also changed. Though the war in Indochina no longer plays an important role in Western politics, some of the most important movements on the current scene reflect the clash of Materialist and Post-Materialist world views—among them, the women's movement, the consumer advocacy movement, the environmental movement and the antinuclear movement. These movements involve questions of whether one gives top priority to economic growth, or to the individual's right to self-realization and the quality of life.

Environmentalism and Economic Growth

Until quite recently, it was taken as self-evident that economic growth was inherently good; though there were sharp disagreements on how its benefits should be allocated, the pro-growth consensus embraced both labor and management, capitalist and communist. Only recently has this assumption been called into question, with the environmental movement holding that economic growth does not always justify the impact it makes on the environment; and with some segments of the movement arguing that economic growth is now becoming either undesirable or even impossible, because of the scarcity of natural resources. When environmentalism raises questions of environmental quality versus economic growth is now becoming either undesirable or even Materialist priorities squarely against Materialist ones. Thus in 1977, among the Materialists in the European Community publics, 36 percent expressed a "very high" opinion of the environmental movement—while 53 percent of the Post-Materialists did so. And while 3 percent of the former claimed to be members of some environmentalist group, 7 percent of the Post-Materialists did so.

It is significant that the environmental movement has *not* collapsed, in the post-1973 setting of severe strains on Western economies due to skyrocketing energy costs that exacerbate inflation and drain away immense sums of capital that might otherwise be invested to produce fuller employment. Despite this economic crisis, and a subsequent backlash against environmentalism, environmental protection standards were not abandoned—on the contrary, they became more stringent after 1973, when the energy crisis became manifest. Currently, cross-pressured groups seem to be wavering, but Post-Materialist support for environmentalism remains firm and the movement continues to win some victories. Stricter limitations were enfored on the mining and burning of coal, and on automotive emissions in the late 1970s. Although the amount of hydroelectric power produced in the United States could be doubled fairly readily, environmentalist opposition has made construction of major new hydroelectric projects next to impossible. In 1980, a proposed Energy Mobilization Board—designed to facilitate the development of energy resources in the face of environmentalist opposition—died in Congress; and millions of acres of land were added to wilderness areas that are largely closed to exploration for natural resources.

But the most dramatic and emotionally charged confrontation between Materialist and Post-Materialist priorities was the struggle over nuclear power. One can conceive of a world in which Post-Materialists favored the development of nuclear power on the grounds that it disturbs the natural environment less than coal mines, petroleum wells or hydroelectric dams, and that it produces less pollution and has a better safety record than conventional energy sources. This is conceivable—but the reality is quite different.

Nuclear power has come to symbolize everything the Post-Materialists oppose. It carries connotations of the bombing of Hiroshima, reinforced by fears that nuclear power plants might facilitate the spread of nuclear weapons. Based on complex technology, nuclear power was developed by large corporations and the federal government, in the name of economic growth. Post-Materialists were disproportionately active in the antiwar movement, tend to be suspicious of big business, and big government—and give low priority to economic growth. They form the core of the opposition to nuclear power. And, despite the current energy crisis, opposition to nuclear power plants has not died away—on the contrary, it has brought the development of nuclear power almost to a halt in the United States, West Germany and several other countries. In 1968, nuclear power produced less than one percent of the electricity used in the United States; by 1978 it was providing nearly 13 percent of all electricity consumed. After the Three Mile Island accident this development halted: in 1979 nuclear sources produced only 11 percent of the nation's electricity. Facing protracted and unpredictable delays, no new nuclear power plants are currently being ordered and many of those already ordered have been canceled. In South Korea, where environmental and antinuclear groups have virtually no impact, a nuclear power plant can be built in four years. In the United States, construction time now averages about twelve years. It seems possible that the American power plant industry, which led the world until recently, may shut down completely.

Other nations have chosen to pursue the nuclear option vigorously, and a number of them (including Belgium, Sweden, Switzerland, France and Great Britain) are already far ahead of the United States in the percentage of electricity they produce from nuclear sources. By 1985 nuclear power plants will supply about 40 percent of Sweden's electricity and 55 percent of France's electricity. The Soviet Union will quintuple its production of nuclear power between 1980 and 1985. The contrasting American record reflects technological problems to some extent, but above all it is a question of political and ideological factors.

Like the environmental movement, the struggle over nuclear power reflects a clash of world views. For Materialists, the use of nuclear energy is viewed as desirable insofar as it seems linked with economic growth and full employment. For them, highly developed science and industry symbolize progress and prosperity. Among Post-Materialists, nuclear power tends to be rejected not only because of its potential dangers but because it is linked with big business, big science and big government—bureaucratic organizations that are evaluated negatively because they are inherently impersonal and hierarchical, minimizing individual self-expression and human contact. The ideologues of the antinuclear movement argue for a return to a simpler, more human society in which energy is used sparingly, and what is needed comes directly from nature—symbolized by solar power (Nelkin and Pollak, 1981).

Tables 9 and 10 show the relationship between value type and support for developing nuclear energy, among European Community publics and among candidates for the European Parliament. In every country, Materialists are far more favorable to developing nuclear energy than are the Post-Materialists. At the mass level, a majority of the Materialists support the development of nuclear power and a majority of the Post-Materialists oppose it in eight out of nine nations. The differences are even more pronounced at the elite level; a majority of Materialists support nuclear power and a majority of Post-Materialists oppose it among candidates from *every* one of the nine countries. And the percentage spread between Materialists and Post-Materialists is at least 46 points in every country but Italy, where nuclear power has not become a major political issue.

This emerging axis of polarization cuts squarely across traditional left-right lines. On the antinuclear side one finds intellectuals, some socialists —and much of the upper middle class. On the

Table 9. Support for Development of Nuclear Power Among Nine West European Publics, by Value Type, 1979 (Percent)

Question: "Could you tell me whether you agree or disagree with . . . the following proposal? . . . Nuclear energy should be developed to meet future energy needs."

	Britain	France	Germany	Belgium	Italy	Luxembourg	Ireland	Netherlands	Denmark
Materialist	79	77	69	56	57	53	52	59	41
Mixed	75	64	58	57	47	59	45	36	40
Post-Materialist	52	44	46	46	45	36	35	27	20

Source: European Community survey (Barometer 11) carried out in April-May, 1979. Values index is based on top two priorities selected from items E-H, cited in text.

Note: Figures show percentage who said they "agree" or "agree strongly."

pronuclear side, one finds big business—and the AFL-CIO; Gaullists—and the French Communist party. It is not a traditional class struggle, but a polarization based on Materialist versus Post-Materialist values.

One of the most striking features of the nuclear power controversy is the extent to which well-informed members of the public and even competent experts, when exposed to the same body of information, draw totally different conclusions. We believe this reflects a process of cognitive screening in which given facts are retained and weighted in accord with the individual's basic values. Though support or opposition to nuclear power is usually justified in terms of objective costs, benefits and risks, an underlying factor is a clash of world views.

Materialists take it for granted that economic growth is crucial, and weigh the costs and risks of nuclear energy against the costs and risks of alternative energy sources. Post-Materialists take economic security for granted and weigh the costs and risks of nuclear power against various no-cost alternatives—among which, reduced material consumption seems not only acceptable but, to some, actually desirable: insofar as it might lead to a more decentralized, less impersonal society that allows freer play for individual self-expression, it has a very positive image (see Schumacher, 1973; Lovins, 1977; Sale, 1980). Thus the debate over nuclear power is based on contrasting visions of the good society, with pronuclear and antinuclear advocates talking past each other because their arguments are implicitly based on different value priorities. To a considerable degree, each side is insensitive to the basic premises of the other.

Conclusion

A decade's time series data indicate that Post-Materialism is a deep-rooted phenomenon with important political consequences. Persisting in an atmosphere of insecurity, Post-Materialism has

Table 10. Support for Developing Nuclear Power among Candidates to European Parliament, by Value Type, 1979 (Percent)

	France	Germany	Britain	Italy	Belgium	Ireland	Luxembourg	Denmark	Netherlands
Materialist	95	98	77	55	100	54	71	86	64
Mixed	85	65	54	54	40	29	25	14	35
Post-Materialist	49	40	24	44	24	0	0	9	5

Source: European Elections Study survey of 742 candidates for the European Parliament, interviewed in spring, 1979; respondents' value type is based on top two priorities selected from items A-L cited above.

Note: Figures show percentage who said they "agree" or "agree strongly." The question about nuclear energy included an "in-between" option (not proposed to the mass publics) that was selected by from 6 to 26 percent of given national samples: only in The Netherlands, Denmark and Luxembourg were pluralities opposed to developing nuclear energy.

come to manifest itself in new ways under new circumstances; what was a student subculture in the 1960s has evolved into the ideology of The New Class. And conflict between those seeking Materialist and Post-Materialist goals has become the basis of a major dimension of political cleavage, supplementing though not supplanting the familiar polarization between labor and management.

There are ironies on both sides. For generations, predominantly Materialistic elites took it for granted that nature had infinite resources to withstand consumption and environmental pollution, for the sake of industrial development. But growth did *not* inaugurate the "Politics of Consensus in an Age of Affluence" (Lane, 1965). Instead, after a certain lag, it led to the emergence of new sources of discontent while a blind emphasis on growth that depended on cheap oil, undermined its own future.

More recently, some Post-Materialist elites adopted a mirror image of this view, taking it for granted that industry had infinite resources to support taxation, and regulation—sometimes in a needlessly punitive spirit. The irony here was that in the long run, Post-Materialism is contingent on material security; the insecurity of recent years arrested its growth and gave impetus to a renewed emphasis on reindustrialization and rearmament.

The rise of Post-Materialism was accompanied by a wave of legislation designed to advance the cause of human equality, raise social welfare standards, and protect the consumer and the environment. There were pervasive changes in national priorities. Prior to 1965, over half of the American federal budget was spent on defense and only about one-quarter on health, education and welfare. By the end of the 1970s, these proportions had been almost exactly reversed. That reallocation of resources is now under attack.

It would be gratifying to be able to identify one side as totally right and the other as utterly wrong. Unfortunately it is not that simple. Both Materialist and Post-Materialist goals are essential elements of a good society, and neither emphasis is automatically appropriate, regardless of circumstances. A healthy economy, and defense forces adequate to deter attack are essential to the realization of *both* Materialist and Post-Materialist goals. But they are not the only legitimate political concerns. Beyond a certain point, a military buildup tends to generate countermeasures. The Soviet Union is militarily stronger than ever before, but faces suspicion and opposition from China, Afghanistan, Poland, the West and much of the Third World. And beyond a certain point, material production produces growing social costs and a diminishing payoff. When there are two cars in every garage, a third adds relatively little; a fourth and a fifth would be positively burdensome. From this perspective, a shift to Post-Materialist priorities, with more time, thought and resources going into improving the social and natural environment, is simply a rational response to changing conditions.

A statesman's task is to seek a reasonable balance between a variety of social goals. That task is complicated in any case, but all the more so if the goals themselves can change.

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The Paradoxical Nature of State Making: The Violent Creation of Order

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The central argument of this paper is developed as a criticism of a widely accepted interpretation of collective violence in new states. It is shown that instead of indicating political decay, violence in these states is an integral part of the process of accumulation of power by the national state. To the degree that this power accumulation is necessary for the imposition or maintenance of order, collective violence also indicates movement towards political order on a new scale. Admittedly, our evidence is far from definitive. Nevertheless, it consistently contradicts the interpretation of violence as political decay and supports our interpretation of violence as a usual feature of the process of primitive accumulation of power.

> The decisive means for politics is violence. . . . Anyone who fails to see this is, indeed, a political infant.
>
> Max Weber

Order or Decay?

It was not all that long ago when writers on political modernization were optimistically postulating the existence of a positive, reinforcing relationship between economic growth and the emergence of stable, orderly polities in the developing world. Soon, however, this optimistic equation gave way before the intractable reality of increasing rather than decreasing levels of collective violence and political instability in the Third World. Theorists of political development began to modify and reorient their arguments. Now, rapid socioeconomic change was seen as a breeding ground of discontent, conflict, violence, and political instability. The obvious reality of violence and instability in Third World countries was taken to indicate that instead of developing, these states were, in fact, experiencing political decay.[1]

This reevaluation of the prospects for political development in the Third World shifted the focus of developmental studies away from the dynamics of socioeconomic change and towards questions about the specifically political conditions for order and stability. The argument advanced was that if new states were to develop the capacity to check the violence and instability inherent in socioeconomic modernization then they had to become differentiated, autonomous, centralized organizations with control over sufficient power resources to enable them to enforce order. That is, the more these states increased their power the less violence and disorder they would experience. In this view, state-making is associated with political order and an incapacity to develop state power with violence and political decay. This argument sounds convincing enough. Actually, it is seriously flawed.

By categorically juxtaposing order and decay and interpreting increases in collective violence as solely indicative of movement toward the latter, the argument seriously misrepresents the historical process of state-making. If political scientists who accepted this theory were to look at sixteenth- and seventeenth-century European history, they would no doubt conclude that the whole continent was caught in the grips of a process of political decay. The conclusion would not, of course, be entirely false, but neither would it be entirely true. For beneath the surface of violence, revolt, rebellion, war, and instability, something quite different from political decay was occurring. These were, after all, the centuries during which the proto-national states of Europe were decisively accumulating, centralizing, and concentrating the power resources necessary for effective territorial domination.[2]

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[1] For an exhaustive history of the different views on political development see Huntington and Dominquez (1975). The major work indicating the reorientation of the field is Huntington's (1968, 1971).

[2] For a recent overview of the European experience of state-making see Tilly (1975), especially pp. 3-83, 601-38. Other synthetic works are Bendix (1964, 1968), Organski (1965), Anderson (1974) and Poggi (1978). Also see Carsten (1954), Rosenberg (1958) and Hintze (1975). The French history of state-making has been especially well analyzed. See Goubert (1970), Lublin-

It was a period of primitive central state power accumulation which continued well into the twentieth century. The entire historical process of creating a national state was a long and violent struggle pitting the agents of state centralization against myriad local and regional opponents. Monarchs, princes, lords, bishops, municipal oligarchs, and regional parliaments recurrently and violently confronted one another in a struggle for control over the means of administration and coercion. Peasants and artisans, solidly based in their respective communities, were increasingly mobilized into the fray, sometimes as allies of one or another of the "elite" contenders and sometimes as independent actors resisting the extractions of the proto-states and/or their opponents. As centralizing, war-making state builders increased their resource demands on their populations, the tax, food, and conscription riot often became the harbinger of much larger rebellions pitting nobles and peasants against the monarchical agents of national state centralization. By 1900 there were around 20 times fewer independent polities in Europe than there had been in 1500. They did not disappear peacefully or decay as the national state developed; they were the losers in a protracted war of all against all.

Many of the new states of today are engaged in struggles whose logic is similar to that of the European period of primitive central state power accumulation. The protracted conflicts between centralists and federalists in Mexico, between Java and the Outer Islands in Indonesia, and the linguistic and secessionist struggles of India all evidence antagonisms between central state-makers and subnational collectivities. The theoretical language of "cleavages"—ethnic, religious, tribal—tends to obscure their intimate connections with competitive political conflicts for control over the power resources of the respective territories and populations. Increasing central state claims for resources—for the material means of state-making and domination—intrude into and compete with preexisting structures of rights and obligations which tie those resources to subnational collectivities and/or "polities." Conflict, resistance, and violence are, as they were in Europe, often the result.[3]

Our argument is not that the specific actors, patterns and modalities of conflict in new states, or their outcomes, are the same as they were in Europe. The fact that the European national state system did emerge and that the new states now occupy distinct positions within a developed capitalist world economy are enough to insure that such a parallel is unlikely. Nor are we contending that conflict and violence in the Third World can be reduced to struggles over the concentration and centralization of state power. The only point we are stressing is that a significant amount of the political violence in new states is a function of the conflicts inherent in the process of primitive central state power accumulation. But if this is true, how is it possible singularly and unambiguously to interpret collective violence as indicative of political decay?

The fact is that collective political violence, in and of itself, indicates neither order nor decay. To equate increasing violence with increasing political decay is to adopt an undialectical, historically inaccurate conception of the process of national state making. National state making is a historical process characterized by the creation of political order at a new spatial and institutional level. It involves the redistribution of the political control of power resources away from subnational collectivities and polities toward the central state apparatus. Historically, this centralization of power resources is a violent process which, if successful, leads to the creation of "order" at a new, more expanded level. In this case, then, violence can be seen as indicating a progression toward a political order of a qualitatively different kind rather than as political decay. The task is, therefore, one of correctly interpreting the significance of collective violence in the new states. The question we are posing in this article is in what ways and to what extent is the collective violence in new states a reflection or tracer of the process of national state-making and consolidation?

State-Making and Violence in New States

There is no systematic comparative study of state-making in new states. The available relevant information is spread over a multitude of case studies which do not have the process of centralization as their major focus. The little information we have, however, shows that the march toward the centralization of power has continued implacably in a number of the recently independent states. We know that the leaders of independence were deeply committed to the economic growth and political aggrandizement of their countries, and thus sought to expand the power of the state apparatus inherited from the colonizers. We also know that building an administrative apparatus that could penetrate more deeply into the national

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skaya (1968), Salmon (1975), Coveney (1977) and Mousnier (1979).

[3] Good overviews of the connections between ethnic, religious, linguistic, and other primordial conflicts, and between those and state-making are Wolf (1969), Geertz (1973), Part IV, and Lewis (1974), Parts 2 and 3. For further references see the next section.

territory, and the repressive apparatus necessary to back it up, required a continuously expanding extraction of resources from the population. Such extraction, of course, not only perpetuated the old conflicts of the colonial state but also generated new ones.

Newly established states are likely both to exacerbate old conflicts and to create new ones by financing the expansion of the state apparatus through increases in the tax burden on the major producers of agrarian societies: the peasants. It is well known that the major driving force behind peasant involvement in the wars of independence was the peasants' anger at the immense tax burdens imposed by colonial regimes. Peasants expected to rid themselves of taxes as soon as foreign rulers left. Their hopes were, however, cruelly betrayed by the native rulers who replaced colonial despots. The imperatives of national aggrandizement required resources. Javanese peasants who wanted to "kill the Dutch because they impose[d] taxes" (Scott, 1976, p. 91) were soon to realize they had to kill their "liberators" to free themselves from taxes. Peasants fought back in the same way they had fought colonial states, and for much the same reason they had fought state-makers in seventeenth-century Europe. In the same way as European peasant communities resisted the king's officials, often led by the landed aristocracy, the villagers of the new states united against government claims, this time mostly under the guidance and protection of communist parties or other movements engaged in struggles with the central government.

In talking about India in the late fifties, Myron Weiner notes that one of the things uniting villagers was opposition to government orders they perceived as unjust. Weiner's explanation (1962, p. 146) of how the anger of the rural population at the extractive incursions of the state is linked to the national political struggle illustrates the pattern of conflict we have laid out above:

> In fact, this capacity to unite in relation to the outside world is increasingly being tapped by political parties during general elections that cut across villages. In elections for state legislative assemblies and for the national Parliament, virtually all parties, including the Communists, emphasize those issues which appeal to villages as villages. Thus the Congress Party stresses the beneficial effect of its community development programs, of local irrigation works, new schools, new roads, and other rural improvements. The opposition parties criticize the government for high taxes, high food prices, for inefficient programs of grain procurement, for administrative corruption, and for lack of adequate credit facilities, irritation works, or schools. In West Bengal, as elsewhere, rural demands are increasingly directed at the government not against groups within the rural community. While leftist attacks on *zamidar's, jagirdar's,* and other types of landlords were common in the 1952 elections, the legal abolition of the landlord system in most states between the 1952 and 1957 elections eliminated this important class-struggle issue.

There are, of course, critical differences between the collective action of Asian villagers today and that of European peasants in the eighteenth century. But these differences should not be exaggerated. Almost echoing Weiner's remark is Rude's statement about seventeenth- and eighteenth-century European peasants (1980, pp. 54-55):

> The more common feature of peasant revolt in the "age of absolutism" was the challenge to state or monarch over the payment of taxes rather than to the seigneur overdues and obligations, or even over personal servitude.

One of the crucial differences between eruptions of peasant resistance in new states and that in early modern Europe is that the former are more integrated into national power struggles. While the European aristocracy often took the lead in peasant resistance against the Crown, such resistance remained predominantly local.[4] In new states, however, parties and groups competing at the national level integrate local resistance into national conflicts. Thus, while peasant resistance to state expansion is universal, the political and organizational character of the powerful allies of the peasantry changes.[5] But, whatever the differences in the character of these allies may be, state expansion will always provide sufficient reasons for resistance to a number of local centers of power. As the centralizing and expansionist action of the French Crown of the seventeenth century provoked a violent reaction from the aristocracy, and that of the English Crown from the gentry and the Parliament, the same sort of action unleashes much violence against the central government in new states.

In trying to expand their power, the governments of new states may threaten or displease other centers of power in a number of ways. They may do so simply by failing to retribute favors, thereby having to face retaliation by the offended group. For example, the Moroccan government had to face much hostility from Riffian Berbers when it failed to reciprocate the substantial military help it received from them during the struggle for independence. In a somewhat more drastic vein, the newly formed central government can generate much conflit, and perhaps much violence, by corroding and redefining patterns of control over valuable resources. Such government initiatives typically unleash conflicts around constitutional issues. To the extent that the state apparatus is obviously controlled by a group of different ethnic origin from that of the group who stands to lose from the government's initiatives, these conflicts will also have an ethnic content. By the same token, such issues can also have regional overtones if the group suffering the consequences of government action derives its power from a control over the resources produced in a given region of the political unit. This is precisely what happened in Ghana when Nkrumah attempted to increase state revenues through a new Cocoa Ordinance which fixed cocoa prices for four years in a context of rising world prices. The main opposition to the ordinance came, of course, from the cocoa-growing Ashanti areas. Ashanti cocoa farmers organized a strong political opposition to the central government and its party, the CPP. They pushed for a federal rather than a unitary form of government for Ghana: "Under a federal system, farmers hoped to retain cocoa profits in the region and provide for themselves an economically prosperous, regional basis of power" (Harris, 1975, p. 65; see also Austin, 1964).

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[4] For the dispute about the relative importance of aristocratic leadership in peasant rebellions, see Mousnier (1958) and Porshnev (1963).

[5] For studies of peasant resistance against the state in a variety of times and places see Hobsbawm (1965), Moore (1966), Wolf (1969), Mousnier (1972), Lewis (1974), Scott (1976), Blum (1978), Rude (1980).

The bloody conflict between Java and the Outer Islands of Indonesia is another example of how central governments in new states can promote conflict and violence. In Indonesia, it was in the interests of Sukarno's government to drain resources from the rich Outer Islands to poor Java. Rather than reducing their control, in this case the state was denying the Outer Islands control over the resources they themselves produced. The result was armed revolt (Geertz, 1973; Feith, 1959). Many of these political conflicts can evolve into full-scale wars of secession. For they all in some way involve a potential challenge to the sovereignty of the state. Thus, long-standing political, social and economic grievances of the East Pakistani led them to revolt against the Westerners who controlled the state apparatus (see Merritt, 1969), and the persecution of Ibos by the powerful northerners in Nigeria led to Biafra (see Young, 1976, pp. 460-504).

All of these conflicts are defensive in nature; they are all brought about by the aggressive expansionism of the state. Although they do not necessarily involve violence, in new states they have usually generated a great deal of collective violence. This is so because new states are still involved in the primitive accumulation and centralization of power resources. The result is that these states and their domestic opponents are locked into a vicious circle of increasing violence. Until these states accumulate the amount of power resources that will make the costs of antistate action prohibitive, their opponents will fiercely resist their extractive claims. Since state-makers are unlikely to give up their claims to sovereignty, they will tend to confront their opponents violently to ensure their control over the resources necessary for effective territorial domination. It is only if and when they achieve such domination that the level of violent interactions between the state and its opponents will significantly decline. Only at this point, if the state ever reaches it, will antistate mobilization become extremely costly and ineffective. It will then be much easier for the state to coopt or disregard its opponents' claims. But, of course, this point cannot be reached without the state and its opponents passing through the violent phase of primitive accumulation of power.

Theoretical Specification and Empirical Verification

If our reasoning is correct, we should expect expanding state power in new states to be highly correlated with collective violence: the greater the expansion, the greater the violence. This argument is quite distinct from the dominant theories concerning collective violence in new states. Most theorists have focused on socioeconomic transformations as the major determinants of collective violence in new states (Davies, 1962; Gurr, 1973). In contrast, our theory emphasizes political transformation—especially state-making—as a major cause of collective violence. We are not alone in our emphasis of state formation. Two other writers have done so: Samuel P. Huntington (1968) and Charles Tilly (1973, 1978). But their arguments are very different. While Tilly sees state-making as a violent process, Huntington views it as checking violence. Obviously, on this point, our view is closer to Tilly's. We are not, however, entirely in disagreement with Huntington's argument.

Tilly's argument is based on his investigations into the early phases of state-making in Western Europe, or what we have called the period of primitive accumulation of state power. His conclusion that state-making is a major cause of violence is appropriate for this early phase in the overall process of state formation. As we have already indicated, however, it is unlikely that this relationship will hold beyond the primitive level of state power accumulation. Beyond this level, Huntington's argument that state power operates to reduce the overall level of collective violence is correct. The problem, however, is that he incorrectly applies this argument to states that are still in the primitive accumulation phase of state-making.[6]

The correct formulation, then, is that the extent to which an expansion of state power will generate collective violence depends on the level of state power prior to that expansion. Our hypothesis can therefore be stated in the following terms: other things being constant, the lower the initial *level* of state power, the stronger the relationship between the *rate* of state expansion and collective violence.[7] Generally speaking, this hypothesis implies that new states attempting to increase the power resources of the state are likely to display a higher level of collective violence than old ones because they tend to be at much lower initial levels of state power. We shall now test this hypothesis.

We shall use tax revenues to measure state power. Government tax revenues in themselves are not very good indicators of state power because they also reflect, to a large extent, the wealth of nations. To control for differences in wealth, we shall use as a measure of state power the proportion of national wealth extracted by the state in the form of taxes. This proportion will be labeled "tax ratio." Since an increase in tax ratio is an increase in the state's share of the total resources of a nation, it indicates an increase in the power of the state relative to other centers of power—that is, it indicates an increase in the state's control over the power resources available in any given society. Thus, an increase in tax ratio is a manifestation of a restructuring of power relations, of a change in the balance of power of a society in favor of the state.

Given that our argument implies that new states are more violent than old ones because they are undergoing a process of primitive accumulation of power, we must first show that new states are indeed going through such a process. This can be done by showing that these states have in fact expanded their power after independence but that they are still considerably less powerful than old, consolidated, states. To show this, however, we must first define "new" and "old" states. The term "new states" is usually used in very vague ways. It can be used to denote countries that have achieved independence after World War II or, more broadly, to denote countries which entered their *modern* phase, in all senses of the word modern, during the twentieth century. The latter sense is more common. The author of a widely quoted paper on "new states," Clifford Geertz, uses it in this broader sense (1973, p. 234, n. 1):

> The term "new states" indeterminate to begin with, becomes even more so as time passes and the states age. Though my main referent is the countries that have gained independence since World War II, I do not hesitate, where it suits my purposes and seems realistic, to extend the term to cover states like those of the Middle East, whose formal independence came earlier, or even those, like Ethiopia, Iran, or Thailand, which in the strict sense were never colonies at all.

We might add that, according to this broader definition, Latin American states can also be defined as "new states."

Now, for both of these senses of the word "new," it can be shown that in fact new states have been, on average, expanding at least as much as old ones, but are considerably less powerful than the latter. To show this we divided the 105 countries for which we have tax series into four categories according to the date at which they became autonomous, if they were colonies at all. The first category contains all nations that became independent after World War II. The second, those who became independent during the nineteenth century or the early twentieth century. The third is reserved to old non-Western kingdoms and empires, like Iran and Afghanistan, who never were colonies in the strict sense of the word. In the fourth category are those European and North American states that were autonomous before the nineteenth century. Table 1 below shows how powerful—that is, how large—was the tax ratio of each of these categories of states both at the date of their independence and in 1975.

The conclusion that "new states" at the time of independence, or in 1950, were at a much lower level of state power than "old Western states" is inescapable. In 1950 almost 70 percent of the latter were very powerful while only 2 percent of those who became independent after World War II were so. None of the old non-Western states was very powerful, and only 22 percent of the states who became independent in the nineteenth and early twentieth century were very powerful. However, the fact that, with the exception of old non-Western states, most new states were of

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[6] It should be noted that Huntington is not unaware of the fact that the process of institutionalization can be an extremely violent one. In his discussion of political change in traditional societies he compares China and Japan, Ruanda and Urundi, the Buganda and the Fulani-Hausa, precisely in terms of the violence of their process of political change. Yet in his theoretical formulations, he only speaks of institutionalization as checking collective violence.

[7] This does not mean, of course, that expanding states, or consolidated ones, do not break down, although this is relatively rare and generally related to international conflict. See Skocpol (1979).

Table 1. Power of Autonomous States (Percent)

Power Measured by Tax Ratio	States Independent after WW II		States Independent during 19th and Early 20th Century		Old Non-Western States		Old Western States	
	1950 or Year of Independence	1975	1950 or Year of Independence	1975	1950 or Year of Independence	1975	1950 or Year of Independence	1975
Low 0-9%	33	9	23	10	71	29	0	0
Medium 10%-19%	65	66	55	45	29	57	34	25
High 20% or more	2	25	22	45	0	14	66	75
N =	(55)	(55)	(31)	(31)	(7)	(7)	(12)	(12)

Source: The components of our tax ratios come from World Bank and UN publications. The data were compiled by A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), Appendix 1 and will be available through the Inter-University Consortium for Political Research.

medium power rather than of very low power shows that much of the primitive accumulation of power was undertaken by the colonizers. Table 1 also shows that although in 1975 "old Western states" were still considerably more powerful than "new" ones, the latter had nevertheless become considerably more powerful. This means that new states had undergone considerable expansions in state power between the date of their independence, or 1950, and 1975. The substantial expansion of new states after World War II can be better seen in Table 2, where it is shown that new states almost doubled their state power.[8]

We must now show that it is the combination of low initial state power and high rates of state expansion in new states that makes them more violent than old states, which also had high rates of expansion but started from a much higher initial level of state power. In other words, we must test the hypothesis that, other things being constant, the lower the initial level of state power, the stronger the relationship between the rate of state expansion and collective violence.

The *World Handbook of Political and Social Indicators* provided our measures of collective violence.[9] We used three indicators. Since we are

Table 2. Average Tax Ratio by Type of State at Year of Independence, or in 1950, and in 1975 (Percent)

Type of State	Tax Ratio at Year of Independence or 1950	Tax Ratio in 1975	N
Independent after World War II	11	17	55
Independent in 19th or early 20th century	14	20	31
Old Non-Western	9	16	7
Old Western	21	26	12

Source: The components of our tax ratios come from World Bank and UN publications. The data were compiled by A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), Appendix 1, and will be available through the Inter-University Consortium for Political Research.

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[8] A comparison between "new" and "old Western" states should be carried out with extreme caution. Additional extraction is much easier from a lower base. Consequently the 5 percent increase for "old Western" states is probably much more difficult than the 6 percent increase of "new" states between 1950 and 1975 (see Table 2).

[9] The data were made available by the Inter-University Consortium for Political Research. The data were

dealing with *collective* violence we used the only two event variables that involved violent interaction between the state and a sizable group of people: riots and armed attacks. These two variables were added and called "violent contention." The third variable is the number of deaths in conjunction with collective violence. The period covered runs from 1950 to 1965; that is, the variables will be the average number of deaths and collective contention between 1950 and 1965. The two last years (1966-1967) of the *World Handbook* series were excluded because the series of economic indicators necessary for our analysis stop in 1965. As for the rate of expansion of state power, the indicator used is the annual growth rate of tax ratio between 1950 and 1965.

Unfortunately, we have complete series for only 31 nations. We could have expanded the number of cases by decreasing the number of years involved. But the 16-year period we chose is already a short one for our purposes. We do not know what is the time-lag between state expansion and a violent reaction to it. We can only say that over a relatively long period of time state expansion will generate collective violence. Thus until further

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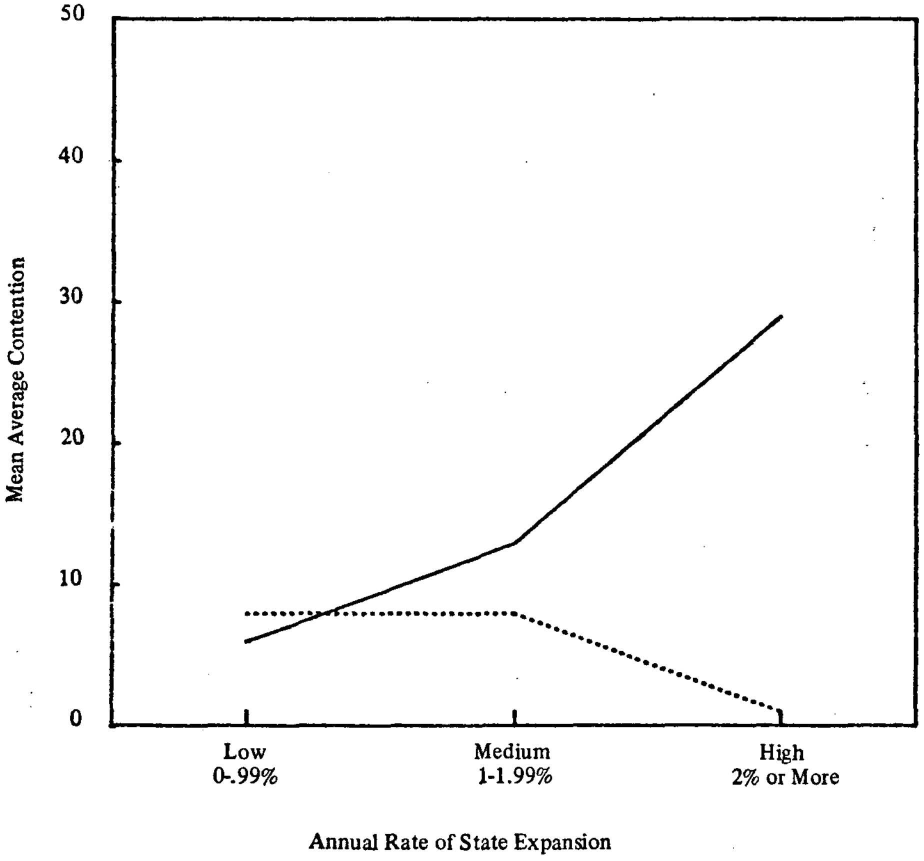
originally collected by Charles L. Taylor and Michael C. Hudson (1976). Neither the original collectors of the data nor the Consortium bear any responsibility for the analyses or interpretations presented here.



Sources: Data on contention are taken from Charles L. Taylor and Michael C. Hudson, *World Handbook of Political and Social Indicators* (Ann Arbor, Mich.: Inter-University Consortium for Political and Social Research, 1976). Tax ratio data from A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), Appendix 1, and will be available through the Inter-University Consortium for Political Research.

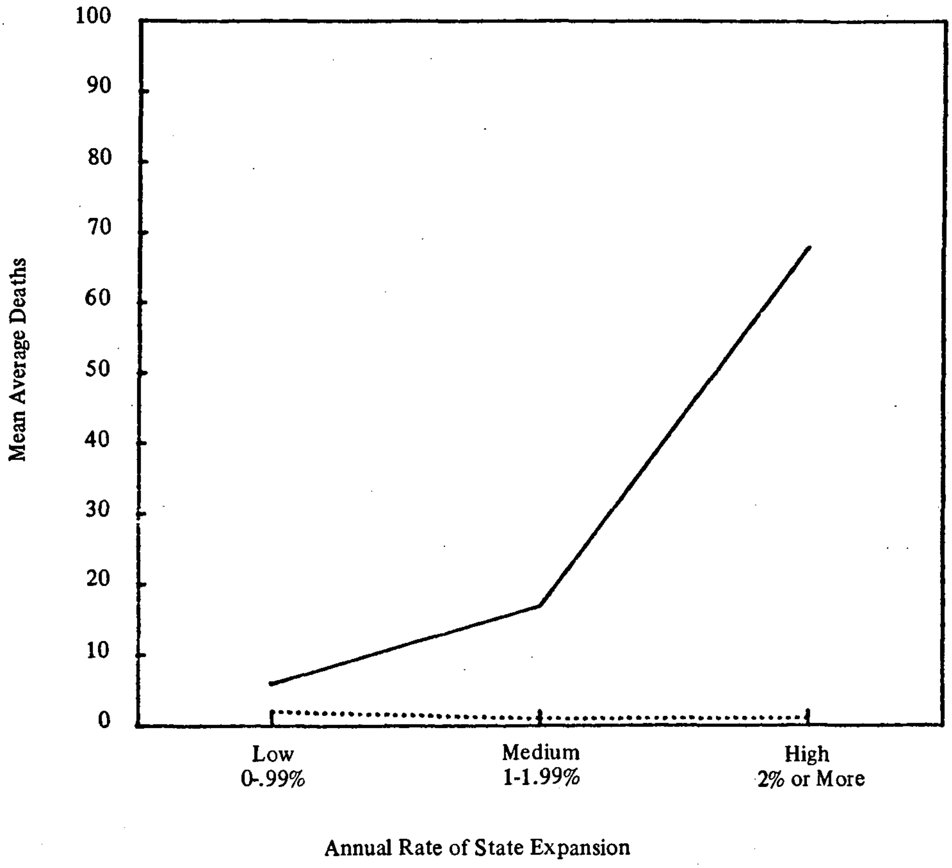
Key: ______ Weak States (Tax Ratio: 0%—15%
. . . . . Strong States (Tax Ratio: 16% or more)

Figure 1. Mean Average Collective Contention by Rate of State Expansion (1950-1965)

evidence is collected we shall have to content ourselves with a rather limited set of countries.

The relationship between the rate of state expansion and our two indicators of collective violence at different levels of initial state power—state power for autonomous nations in 1950—is shown in the graphs below.[10]

Although the limited number of countries raises much doubt as to the conclusiveness of our evidence, the graphs above, together with Tables 1 and 2, clearly indicate that there is good reason to believe that it is the process of primitive accumulation of power that generates much of the violence in new states. In other words, it is the progression toward greater order itself that produces much of the relatively greater violence we find in new states. It could still be argued, of course, that our measure of state expansion is far from pure. After all, economic development involves structural changes, such as major improvements in communication systems, which greatly facilitate the extractive tasks of the state. Thus tax ratios might reflect, to a large extent, the level of



Sources: Data on deaths are taken from Charles L. Taylor and Michael C. Hudson, *World Handbook of Political and Social Indicators* (Ann Arbor, Mich.: Inter-University Consortium for Political and Social Research, 1976). Tax ration data from A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), Appendix 1, and will be available through the Inter-University Consortium for Political Research.

Key: ______ Weak States (Tax Ratio: 0%—15%)
. . . . . Strong States (Tax Ratio: 16% or more)

Figure 2. Mean Average Deaths by Rate of State Expansion (1950-1965)

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[10] We had to exclude from our analysis two countries, for which we had complete series, the Philippines and Colombia, because they had an extremely unusual ratio of deaths per contention. However, even if those countries had been included, our results would remain the same where contention is concerned, although they would be considerably weakened in the case of deaths.

economic development and expansions in tax ratios might therefore reflect economic change. This poses a serious problem for our interpretation. For, to the extent that the tax ratio is an alternative measure of economic development, we would merely be repeating the argument that violence is a function of economic modernization. To verify whether this was the case, we reanalyzed our data on expansion and violence for weak states, this time controlling for the annual rate of growth in GNP between 1950 and 1965. The results are presented in Table 3.

For the limited set of countries analyzed, our results indicate that tax ratios are not the same as the usual indicator of economic modernization. Furthermore, the evidence strongly suggests that the rate of economic development is related to both the rate of state expansion and collective violence in a way that runs contrary to the way postulated by the predominant view on such matters.

In the first place, state expansion seems to produce much more violence than economic growth. When we move from low to high state expansion, the "deaths" figure increases from 5 to 106 and from 16 to 46, while the corresponding movement for rates of economic growth is from 5 to 16 and from 106 to 46; the figures for contention show an equivalent pattern. Thus, not only state expansion produces more violence than economic growth, but the latter also seems to act counter to state expansion under conditions of high state expansion. Rather than state expansion being an antidote for the violence produced by economic modernization, our rather limited evidence shows that it is economic modernization which is the antidote to the violence produced by state expansion. This is much in line with common sense, which has it that the government, or any group, for that matter, will have less trouble cutting a greater slice of a growing pie.

Conclusion

The central argument of this article has been developed as a criticism of the standard interpretation of collective violence in new states. We have shown that instead of indicating political decay, violence in these states is an integral part of the process of the accumulation of power by the national state apparatus. To the degree that this power accumulation is necessary for the imposition or maintenance of order, collective violence is also indicative of movement towards political order on a new scale. Admittedly, our evidence is far from definitive. Nevertheless it at least consistently contradicts the interpretation of violence as political decay and supports our interpretation of violence as a usual feature of the process of primitive accumulation of power.

Since we took existing interpretations of violence as our starting point, we have focused our attention exclusively on the problem of political order in relation to violence. This in no way implies that we share the prevailing commitment to political order as the primary value of political life. To be sure, liberty and justice are at least as important as the achievement of political order. In the absence of effective liberty and justice, order will no doubt always be precarious and, perhaps, undesirable. For, as Saint Augustine remarked: "Without justice, what is government but a great robbery?"[11]

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Table 3. Collective Violence by State Expansion and Economic Growth for Weak States (1950-1965)

	Rate of State Expansion	
Rate of Economic Growth	Low (0-1.99)	High (2 or more)
Low (0-1.99)	5, 7	106, 47
High (2 or more)	16, 12	46, 18
N =	(12)	(8)

Sources: Data on violence are taken from Charles L. Taylor and Michael C. Hudson, *World Handbook of Political and Social Indicators* (Ann Arbor, Mich.: Inter-University Consortium for Political and Social Research, 1976). Tax ratio data from A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), Appendix 1, and will be available through the Inter-University Consortium for Political Research.

Note: Figures represent, respectively, mean average deaths and mean average collective contentions.

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[11] Quoted in Lane (1958).

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America's *Departments* of State: Irregular and Regular Syndromes of Policy Making

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This article (1) sketches a general explanation for the growth of coordinative machinery and of irregular personnel in modern governments; (2) identifies both general and specific reasons for this phenomenon in the United States with special reference to foreign policy making; (3) identifies within the American foreign policy-making context the modal characteristics of irregular and regular syndromes of policy making, and the conjunction between personnel and institutional base; (4) traces the implications arising from these different policy syndromes; and (5) evaluates some proposals for improving the coherence and knowledge base of American foreign policy making. The problems of defining foreign policy authority, assuring an integrated perspective, and effectively using specialized expertise are best seen in terms of the larger problem of governance in Washington against which all proposals for reform must be abraded.

The United States possesses two foreign ministers within the same government: the one who heads the Department of State, and the one who is the assistant to the president for national security affairs. The former heads a classically contoured bureaucracy. Proximate to him are appointed officials, often with substantial foreign policy experience. At greater distance is a corps of professional foreign service officers (FSOs). Beneath the national security assistant, on the other hand, is a smaller professional staff of somewhat variable size (ranging in recent times from about three dozen to slightly over 50) whose members typically are drawn from universities, other agencies, and research institutes.

This latter group—the National Security Council staff—is the institutional embodiment of White House aspirations for imposing foreign policy coordination. Its "director," the president's assistant for national security affairs, in recent years has come to be seen as the president's personal foreign policy spokesman as well as an influential molder, and sometime executor, of his policy choices. Though, at least publicly, the overt role of the president's national security assistant has been diminished in the Reagan administration relative to the prominence it attained during the Nixon and Carter presidencies, a common perception is that, since the Kennedy administration, policy power has drifted steadily from the State Department to the president's team of foreign policy advisers (Campbell, 1971; George, 1972a; Destler, 1972a, 1972b, 1980; Allison and Szanton, 1976). If perceptions govern, this alone may constitute sufficient evidence of such a drift. Beyond perception, however, there is unmistakable evidence of growth in the role of the national security assistant (who postdates the founding of the National Security Council itself), and in the size and character of the NSC staff. Since McGeorge Bundy's incumbency, and especially because of the Kissinger and Brzezinski periods, the assistant to the president for national security affairs has become a visible public figure in his own right (Destler, 1980, pp. 84-85). In general, his role has evolved from one of coordinating clearance across departments to one of policy adviser. Similarly, the NSC staff itself has grown greatly, boosted especially during the Nixon administration. It is less and less composed of graying and grayish anonymous career foreign service officers, and more and more composed of foreign policy intellectuals and prospective high-fliers, many of whom are drawn from America's leading universities.

I do not mean to imply that the presidential foreign policy apparatus and the State Department always or even usually clash, nor that they have wholly overlapping functions. Nonetheless, it is clear that the NSC, at least in form, is today something far beyond what it was in Truman's time or in Eisenhower's. To some degree Truman,

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Christine Dodson, former staff secretary to the National Security Council, and Phyllis Kaminsky, NSC information officer, responded kindly to my requests for documentation. I am especially grateful to Cole Blasier, Paul Hammond, and Robert Putnam with whom I have had extensive discussions regarding ideas presented here. All three combine ideas with experience in the foreign policy and national security areas. In the spirit of the "irregular," they encouraged me to pursue my ideas; in the spirit of the "regular," they doused many of them, perhaps too few. They, of course, bear no responsibility for the contents of the article. I am enormously indebted also to one anonymous reviewer of an earlier rendition of this manuscript for efforts beyond the call of duty. If not all of his suggestions are incorporated here, they were all carefully considered, and my conclusion almost always was, "He's right about that."

protecting what he believed to be his prerogatives, held the then-nascent National Security Council at arm's length as an advisory forum. Eisenhower, on the other hand, employed it frequently as a collegial body, one whose statutory members and staff were also, in some measure, representatives of their departments (Hammond, 1961; pp. 905-10; Falk, 1964, pp. 424-25). Since then, the role and character of the NSC staff, and especially that of the national security assistant, have mutated. This evolution into a role not originally envisioned for the NSC or the then special assistant (executive secretary in Truman's time) has notable consequences for policy making.

Even within the constricted sphere of executive forces on policy making, the foreign policy process involves a complex of actors and not merely a bilateral relationship between the NSC and the State Department. Although the State Department's position has been most eroded by the policy role of the NSC, neither it nor the NSC is a monolithic force. The national security assistant and the NSC staff are *not* the same actors, nor necessarily of common mind. Similar cautions are even more necessary to describe relationships between the secretary of state and the foreign service professionals of the State Department. A significant difference in intraorganizational relationships, however, is that the national security assistant to some extent selects his own staff, whereas the secretary of state has a department to manage and an established subculture that exists well below the level of those whom he selects. If the NSC staff is more nearly the creation of the national security assistant, the secretary of state, unless he divorces himself from the department, is more likely to be seen as its creation. This difference provides one of these actors with considerable strategic advantages in influencing the views and decisions of presidents.

As clearly as this somewhat ambiguous distinction permits, the NSC (and the national security assistant) and the State Department (though not necessarily the secretary) have come to embody, respectively, the differing commitments given to the roles of "irregulars" (those not bound to a career service) and of "regulars" (members of a career service) in the policy process. The correlation is quite far from unity, of course. There are mixes of personnel and outlooks within each organizational setting, but there are characteristically different career lines and perspectives as well. Above all, each setting provides for different roles. The operational responsibilities of the State Department give it the advantages of detailed knowledge and experience, and the political disadvantage of lacking an integrated world view. The NSC, on the other hand, is less constrained by the existence of operational responsibilities, by distance between it and the president, or by the communications complications typical of large hierarchically structured organizations. Its sterling political assets, however, are offset in some measure by the disadvantages of removal from day-to-day detail and highly specialized expertise. The differences between these organizational settings, to be sure, are quite significant. The NSC is a fast track. In contrast, the State Department can be a ponderously slow escalator. One setting is oriented to solving problems, the other to raising them. One is more oriented to attaining a bottom line, the other to journeying down a bottomless pit. In sum, the presidential foreign policy apparatus largely exhibits the advantages and disadvantages of an organ that is staffed to some degree by irregulars, and which is not charged with line functions. The State Department, in the main, illustrates the advantages and disadvantages of a hierarchically structured organization responsible for implementation, and which, therefore, tends to have a regular's orientation.

My objectives in this article then are (1) to sketch a general explanation for the growth of both coordinative institutions and of "irregular" personnel in government; (2) to identify both general and specific reasons for this phenomenon in the United States with respect to the official foreign policy community, in particular the tendencies toward Executive Office centrism; (3) to identify, within the foreign policy context, modal characteristics of the irregular and regular syndromes of policy making, and in so doing, to discuss the conjunction between personnel and institutional base; (4) to trace the implications of these different policy syndromes; and (5) to evaluate some proposed solutions to the problem of both resolving foreign policy-making authority and of organizationally synergizing the "irregular" and "regular" syndromes in foreign policy making. Finally, I conclude by suggesting that the problem of defining foreign policy-making authority in American government is but an element of the larger problem of governance in Washington. Whatever specific palliatives emerge need to be fully grounded in these sobering facts.

The Quest for Policy Integration

The growth of coordinative institutions in modern governments and the growth in importance of irregular staffers in government are not the same thing, but they are traceable to the same sources, namely, the need to compensate for the inadequacies of traditional ministries in absorbing the policy agendas and perspectives of the central decision-making authority within the executive.

The massive expansion of policy agendas themselves—"overload," as Klein and Lewis (1977, p. 2) call this phenomenon—is the signal cause of efforts to overcome the parochialness of the ministries and their civil servants. Problems of assimilation clearly have multiplied as governments pursue more and more complex, frequently conflicting objectives (Rose, 1976a; Neustadt, 1954). The forms taken by these coordinative mechanisms have varied across both political systems and policy arenas. The extent to which they have been composed of irregulars has similarly varied.

The more feeble the gravitational pull of directional authority in government, the more necessary it becomes to institutionalize coordinative functions. In Britain, the relatively strong pull of cabinet government, and the doctrine of ministerial responsibility, means that the interface of politics and policy often takes place within the ministries themselves. There, irregulars are usually planted directly in the ministries. In the Federal Republic of Germany, the gravitational pull of cabinet government is substantially weaker, and the activism of the *Bundeskanzleramt* (the Chancellery) is greater than that of the British Cabinet Office (Dyson, 1974, pp. 361-62). In the case of the United States where the gravitational pull of political forces is exceedingly weak, mechanisms to achieve policy integration abound not only in the Executive Office of the Presidency, but even throughout Congress. The development of these mechanisms throughout the EOP is particularly intriguing in view of the fact that the American line departments are already well saturated with officials whose political pedigrees have been carefully checked out. American administration, as is well known, is laden with irregulars at some depth beneath the cabinet secretary, yet even this has often been considered insufficient to attain presidential control and integration over policy (Nathan, 1975; Heclo, 1975).

This, of course, brings us to the central issue, which is whether the quest for policy integration, defined as comprehensive control over vital policy objectives, can accommodate expertise defined in terms of specialized knowledge. The dilemma, as Paul Hammond once observed, is this (1960, p. 910):

> While the mind of one man may be the most effective instrument for devising diplomatic moves and strategic maneuvers and for infusing . . . creative purpose, its product is bound to be insufficient to meet the needs of the vast organizational structures . . . which are the instruments of foreign policy.

The growth of integrative machinery has brought to the fore officials who sometimes differ from their counterparts in the operating agencies.[1] At least as important, though, is that they are provided substantial policy-influencing opportunities without equivalent operational responsibility. To the extent that "central" staff agencies have challenged more traditional bureaucratic sources of policy, they have merely reflected the perplexing problems that nearly all modern democratic governments face in both integrating and controlling policy objectives, and in rendering them politically acceptable.

From these more general observations, I wish to take up the special case of the National Security Council and the asssistant to the president for national security affairs as a remarkable example of how the facilitating function evolved into far more heady activities. This evolution also starkly illustrates the advantage of a staff agency at the expense of the traditional operating agencies. Put another way, it reflects the advantages that "irregulars" often have over "regulars."

From Manager to Competing Secretary of State

From its inception in 1947, the National Security Council was designed to be a high-level policy review committee rather than a strictly staff operation (Sapin, 1967, p. 84). As a mechanism for arriving at major policy decisions, however, a support staff quickly emerged underneath the statutory membership of the NSC. Indeed, until the Eisenhower administration came into power in 1953, there was no overall coordinator who had immediate access to the president. In 1953, however, Eisenhower appointed a special assistant to the president for national security affairs whose responsibilities, among others, entailed playing an executive director's role with the NSC staff. An indication of how far the function of the president's national security assistant, and that of the NSC staff as well, has diverged from the original coordinating and facilitating function is the fact that it takes a monumental effort to recall who these presidential assistants were.[2]

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[1] Campbell and Szablowski (1979) note, for instance, that senior officials in the Canadian central coordinating agencies differ from the main-line civil servants in the traditional line ministries in that they are more likely to have entered laterally rather than to have moved upward through the civil service system.

[2] From earliest to latest in the Eisenhower administration, they were Robert Cutler, Dillon Anderson, and Gordon Gray. During the Truman administration there were two executive secretaries of the NSC. Each, Sidney Souers and James Lay, reflected the "neutral competence" ideal.

Why has the national security assistant and the NSC staff moved from this relatively modest, if necessary, role to one which frequently has vied with the secretary of state and the State Department for foreign policy-making influence? At the outset of the Carter administration, for example, a sympathetic article referred to the NSC staff as the "other cabinet" (Berry and Kyle, 1977). There are numerous answers to this question, of course. At bottom, though, the "many" reasons are made particularly compelling by the peculiar political culture of Washington politics—an inheritance in part of extravagant institutional disaggregation.

It is true, of course, that whatever clout the national security assistant has exists only at the sufferance of the president (Art, 1973; Destler, 1977). Presidents can make or break the role of their national security assistants as policy advocates. They *can* minimize the visibility of their assistant; they *can* play down the substantive functions of the NSC staff relative to the State Department, for example. There are obvious manipulables in the relationship between America's "second State Department" and the White House, but the norms that have been established now seem firm in spite of the present and perhaps momentary diminution of the NSC role in the Reagan administration. The tendency to shift from central clearance to central direction has helped give the NSC apparatus and, above all, a policy-advocating national security assistant, an unusually important role. In the Reagan administration, Richard Allen has proclaimed his role model to be that of Eisenhower's anonymous special assistant, Gordon Gray. But Allen's own prior roles largely have been advocacy and advisory ones, rather than managerial or facilitative ones (Smith, 1981).

Overload as an Explanation. Understandably, the National Security Act of 1947 which set up the National Security Council was enacted at the beginning of America's postwar eminence as the leading Western power. The role of global power with far-reaching responsibilities produces a busy agenda, and the busier that agenda the more the management of policy and of advice becomes important. According to a relatively recent report prepared for the president, there have been at least 65 studies of the U.S. foreign policy machinery since 1951 (*National Security Policy Integration,* 1979, p. 49). This abundance of studies bears witness to the great diversity of actors with some share of the foreign policy pie, to continuing problems of coordination between them, and to their reputed lack of responsiveness to the president. How, under these circumstances, is a president to make decisions without some final filter that reduces unmanageable complexity to at least endurable perplexity?

Undoubtedly, in an age of instant communication, some of the present NSC apparatus would have had to have been invented did it not already exist. Working out statements with counterparts in the Elysée, the Chancellery, or 10 Downing Street before the principals are themselves engaged is the kind of task that may need to be located close to the head of government. However, the enhanced role of the national security assistant over the past two decades (Destler, 1980) makes it unlikely that these tasks are sufficient to satisfy policy drives created by recent organizational practices.

Institutional and Organizational Explanations. "Overload" explains the existence of coordinative mechanisms such as the NSC. It does not, however, explain the transformation of a once-anonymous role with a small staff to a prominent contender for policy-making power in foreign affairs.

Because government in Washington is as unplanned as the society it governs, criticisms of the foreign policy-making machinery overwhelmingly recommend organizational reforms (Campbell, 1971; Destler, 1972b; Allison and Szanton, 1976). As with virtually all governmental activity in the United States, fragmentation also characterizes the process of foreign policy decision making. Centrifugal tendencies begin at the top levels of American government, induced in part by the absence of effective mechanisms for cabinet decision making.[3] Lack of clarity at the top molds bureaucratic tendencies below. Thus, while the problems of bureaucratic politics exist everywhere, they are made more obvious by unclear boundaries of authority, by the fractionation of power centers, and by the ready availability of the press as a resource in policy struggles. Contemporary Washington epitomizes these conditions. It is not difficult, therefore, to find targets for reform.

Despite repeated calls for its resuscitation, the cabinet is to the functioning of American government what the appendix is to human physiology. It's there, but no one is quite sure why.

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[3] This, of course, is a by-product of the presidential system. Ironically, the Eberstadt Report which set forth the rationale for the National Security Act and, thus, the NSC, apparently was motivated by a desire to create a high level British type cabinet committee. As Hammond notes (1960, p. 899): "The Eberstadt Report assumed that the proposed National Security Council could be a kind of war cabinet in which the responsibilities of the President could be vested. . . . The premise arose . . . out of an inclination to modify the Presidency as an institution."

Whatever initial presidential intentions may be, presidents soon learn that cabinet meetings are mainly for public relations benefits rather than for decision making. They also learn another lesson of particular importance in Washington, namely, that the probability of leaks to the press which may foreclose presidential options is geometrically expanded by the number of participants involved. Later I will discuss how an "information-leaky" environment, unique to Washington among world capitals, estranges presidents from their cabinet departments. For now it is useful merely to indicate that the extreme splintering of responsibilities means that presidents with innovative intentions will be desirous of centralizing in the White House that which is otherwise uncontrollable or unresponsive to them.

All leaders are apt to demand more responsiveness than they can or even ought to get. But American presidents crave responsiveness in part because so little is obviously available to them. Large organizations, and especially those that are highly professionalized, develop definable subcultures and resist intrusions from inexpert outsiders. Regardless of what it is that presidents order the first time, there is a strong tendency for them to be served fudge—or jelly, to employ the culinary metaphor used by President Kennedy. While this frustration is not peculiar to the foreign policy and national security agencies, foreign policy matters are often far more central to what it is a president, or a prime minister for that matter, must attend to (Rose, 1976b, pp. 255-56; 1980, pp. 35-38). Typically, too, there is less legislative direction of the foreign policy organs than of departments having primarily domestic responsibilities or impacts.

Among the agencies involved with foreign policy, moreover, the State Department largely deals with political analysis, impressionistic evidence, and judgment. Since politicians who become presidents are likely to defer to no one when it comes to making what are essentially political judgments, the vulnerability of the State Department becomes apparent. It is not only that the department moves slowly that frustrates presidents; it is often the message that it delivers that leads them to despair (Silberman, 1979).

In addition to its distance from the White House, a problem which to some extent affects all line departments, the culture and technology of the State Department are also factors in its organizational disadvantage. These factors interact with, indeed greatly exacerbate, its distance problem. The State Department is a regular organization *par excellence* with a highly developed professional subculture. The stock in trade of the regular foreign service officers, granted individual differences among them, is a large supply of cold water with which to dash ideas that emanate elsewhere or which challenge prevailing professional perspectives. In the words of one sympathetic observer:

> The most useful service that a senior State Department official can perform in a policymaking role is to douse the facile enthusiasms of administration "activists" in the cold water of reality. But most of them bring so little energy and skill to this task that they merely project an image of negativism (Maechling, 1976, pp. 11-12).

Put somewhat more generally, "Political appointees seem to want to accomplish goals quickly while careerists opt to accomplish things carefully" (Murphy et al., 1978, p. 181).

As a citadel of foreign service professionalism, the State Department is an inhospitable refuge for ideas and initiatives blown in from the cold. "It's all been tried before" is a refrain that may characterize the responses of professional bureaucrats whatever their substantive craft, but it is one that is at the heart of the department's perceived unresponsiveness.

Ironically, in this light, the professional subculture of the foreign service, as some have noted, ill prepares foreign service officers for the rough-and-tumble of bureaucratic politics (Destler, 1972b, pp. 164-66; Maechling, 1976, pp. 10-12; and even Silberman, 1979). Indeed, the recruitment of FSOs traditionally has made them America's closest facsimile of the British administrative class (Seidman, 1980, pp. 144-47). This is manifest also in operating style, a style characterized as one of "alert passivity" (Allison and Szanton, p. 126). While American bureaucrats in the domestic departments readily adopt the role of advocate to a far greater extent than their European peers (Aberbach, Putnam and Rockman, 1981, pp. 94-98), FSOs tend to be more like British bureaucrats, defusing programmatic advocacy so as to maintain the flexibility necessary to deal with the differing priorities imposed by new leaders. Unlike their colleagues in the domestic departments, officials in State lack domestic constituencies to help them weather episodic storms. In addition, the foreign service is oriented to serving abroad. The cost of this absorption is a lack of sophisticated political understanding of the policy-making machinery. In a system in which boundaries of authority are remarkably inexact, FSOs tend to lack both skills and bases for effective bureaucratic infighting—a considerable disadvantage.

As noted, the modal technology of the State Department is soft and impressionistic, and thus endlessly vulnerable. This helps to explain why the

State Department is especially apt to be victimized. For as a former department official comments:

> New presidents and their staffs soon start to search for opportunities for leadership, areas in which to demonstrate that the President is on top of things and making policy. When this game is played, the loser is almost invariably the State Department and not, for example, the Pentagon. . . . To do more than scratch the surface of a few front page military issues would require a much larger White House staff than any President would want to contemplate. Foreign policy, on the other hand, is largely a matter of words, and the President and his staffers can step in at any time and put the words together themselves (Gelb, 1980, p. 35).

Once the staff has been constructed to oversee policy proposals, the next step toward advocacy seems nearly ineluctable unless the president is fully and unequivocally committed *in combination* to the secretary of state as the principal foreign policy maker *and* the State Department institutionally as the principal source of foreign policy advice—a combination that almost necessarily eliminates skilled policy entrepreneurs such as Henry Kissinger from the role. Why this combination of conditions is first unlikely to happen, but difficult to sustain if it does, is the question that needs to be addressed. To do so requires an exploration of the Washington political culture.

The Political Culture of Washington as an Explanation. To explain the transformation of the NSC from a central clearance mechanism and a long-range policy planning one to an active center of policy making requires a focus on institutional and organizational features such as those we have just discussed. Yet the peculiar climate that pervades government in Washington helps to explain these institutional and organizational operations. For the distinguishing characteristic of government in Washington is its near-indistinguishability from politics in Washington. While politics in the capitals of all democratic states mixes together a variety of interests—partisan, pressure-group, bureaucratic, regional, and so forth—the absence of party as a solvent magnifies the importance of other interests. Above all, the overtness of the bureaucratic power struggle is likely to be in inverse proportion to the intensity and clarity of the partisan struggle.

Confronted with singular responsibility and inconstant suport, presidents are often driven to managerial aspirations over "their" branch of the government. Sooner or later they sense that at best they are confronted with inertia, at worst, opposition. Rarely can they rely consistently upon their party for support, especially if they are Democrats; rarely too can they assume that their cabinets are composed of officials who are not essentially departmental emissaries. Cabinet ministers everywhere, of course, are departmental ambassadors to the cabinet. All ministers find it convenient, if not necessary, at some time to promote departmental agendas pushed from below. The late R. H. S. Crossman's assertion that "the Minister is there to present the departmental case" is universally true (1972, p. 61), yet he also observes that an American cabinet is only that—an aggregation of departmental heads (p. 67).

The basic themes of American governmental institutions are distrust and disaggregation. Together, they fuel suspicion. Presidents often come to divide the world into "us" and "them." "They" typically cannot be relied upon. "They" will be seen as torpid, bureaucratically self-interested, and often uncommitted or skeptical of presidential initiatives. Above all, "they" will be seen as an uncontrollable source of hemorrhaging to the press.

Unmediated by any tradition of, or basis for, a cabinet team, distance defines "us" and "them." There are always winners and losers in executive politics everywhere, but the more ambiguous the boundaries of authority, the less clear the adjusting mechanisms by which the winners and losers are determined, and the more pervasive the involvement of the media in policy struggles (a largely American phenomenon), the more ferocious the struggle. Under these conditions, the department heads will tend to lose ground to the White House because whatever advantages in autonomy distance permits, the more obvious are the disadvantages in accessibility.

Washington is a capital as obviously open as Moscow is obviously closed. The intimate involvement of the prestige press in internecine executive policy debates is legendary. Little remains confidential in Washington for very long, at least insofar as the exposure of confidentiality can assist any of the policy contestants. The lifelines connecting presidents to the cabinet departments are longer and perceived to be more porous than those that link presidents to the Executive Office. This perception undoubtedly is fortified by the belief that under most circumstances cabinet secretaries would as soon push their departmental perspectives or even their own special agendas than those of the White House. The secretary of state is not immune from this. Despite the "inner" role of the secretary of state (Cronin, 1975, pp. 190-92), *to the extent that he is perceived within the White House as someone who presses the interests and perspectives of the foreign service regulars, he is apt to be written off as ne of "them."* The case of William Rogers is instructive in this regard, and even more so is that of

Cyrus Vance who began in office with strong presidential support for his power stakes.

Although one must beware of self-serving tales that dribble *ex post facto* to the prestige American press from disgruntled ex-officials, the evidence, however partial it may be, is that leaks to the press are more likely to be blamed on the cabinet departments than on the executive office staff itself. A report in the *Washington Post,* for instance, indicates that after President Carter severely chastised noncareer and career State Department officials in early 1979 for suspected press leaks regarding policy toward Iran, Secretary Vance pressed upon him the view that the State Department was being unfairly singled out as a source of leaks that were regularly occurring everywhere, especially from within the NSC staff. The president's response, according to the report, was to meet with his national security assistant and several of his senior staff members and request them to smooth their relations with their counterparts at the State Department (Armstrong, 1980). To officials at State, the president threatened; to NSC officials, the president cajoled. "Us" versus "them," in other words, was not unique to the Nixon administration (Aberbach and Rockman, 1976).

The isolation of presidents from their cabinet departments, the absence of a common point of meaningful political aggregation—all of this within the information-leaky environment peculiar to Washington among world capitals—is a ready stimulant to the "us" versus "them" outlook that commonly develops in the White House, and in the departments as well. Distance and distrust are promoted on both ends of the tether line connecting departments to the White House. Departmental frustrations are often exacerbated by presidential distrust of bureaucratic institutions in an antibureaucratic culture. American politicians who enter through the gates of the White House have neither learned to endure the frustrations that arise through a slow and steady apprenticeship in party politics such as is found in Britain, nor to appreciate by virtue of living in their midst the skills and qualities that professional civil servants bring to government.

Because it contains memory traces from the past, bureaucracy is the enemy of novelty. Memory imposes constraint, while presidents typically want to make their mark as innovators.[4] Presidential frustrations derive, therefore, from the incapacity of large organizations to be immediately responsive to presidential wishes, and from the tendency of such organizations to protect their interests and core technologies from presidential intrusion. On the other hand, departmental frustrations arise when departments become the victims of imagined nonresponsiveness to presidents, as related in a recently revised version of the trade, proposed by the Soviets during the 1962 Cuban missile crisis, of American Jupiter missile bases in Turkey for those installed by the Soviets in Cuba.[5]

Thus far, I have outlined generally why presidents in America tend toward White House centrism—that is, why they seek to build a policy-making apparatus around them rather than relying exclusively upon the cabinet departments. I have not attempted to explain exhaustively this drive toward centrism, nor its ebbs and flows across particular administrations. My concern is with the trend line rather than the perturbations within it. Some aspects of the drive toward centrism are undoubtedly largely idiosyncratic, having to do with particular presidential styles and personalities within administrations. There are some reasons too that are probably universal, for example, the growth of technological capacity for central control, and some that are speculative, for example, hypothesized imperatives of leaders to try to exert control over policy without comprehending the mechanics—to reach, in other words, a bottom line without much concern for the algorithm. Of the reasons I explicitly cite, however, one—the increased agenda of governments—can be found in all modern democracies, and has resulted in efforts to devise coordinative machinery. The other reasons I have identified are more system-specific, and are rooted in institutional and cultural considerations. A dispersed policy universe generates needs for greater centrism. The weaker the pull of political gravity, the more the emphasis upon central staffing. Thus, according to one report, the load on central staff personnel in the EOP (at least during the Carter presidency) is immense when compared with staff counterparts in other nations (Campbell, 1980, p. 22). This apparently reflects White House obsessions for detailed policy control in an environment in which such control is as elusive as it is expected.

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[4] A recent study of organizational memory development among three EOP agencies, for example, finds that the NSC consistently has the least cross-administration continuity as measured by several indicators (Covington, 1981). The author of this report concludes that organizational continuity reflects presidential detachment, whereas lack of memory reflects intense presidential interest.

[5] As Barton J. Bernstein (1980, p. 103 n.) observes from his study of recently declassified materials regarding this episode:

> A chief executive may often express preferences (not orders) for policies, and that he may sincerely reinterpret them as *orders* when his own inaction leaves him woefully unprepared in a crisis. In this way, a president can place blame on a subordinate, and other aides who listen to his charges tend to believe that the president actually issued an order, and not simply stated a wish or a hope.

For a general review of this incident, see Bernstein (1980), and also Hafner (1977).

Foreign Policy by Irregulars: White House and Departmental Settings

The conditions that make American presidents turn to staff at the White House rather than bureaucrats, or even their appointees in the departments, undoubtedly characterize all policy sectors. Departmental appointees who have strong links to the career subcultures within their departments are often viewed with suspicion at the White House. They will be seen as advocates of parochial interests. Officials at the White House want presidential objectives to be "rationally" managed. Officials in the departments, on the other hand, want "rational" policies as they define them. This difference in perspective exists everywhere, and is by no means a peculiar characteristic of White House-State Department relations. What is peculiar about this particular relationship, however, is the extent to which the foreign service regulars are cut adrift from other sources of support in the political system and within their own department. Unlike their counterparts in the domestic agencies, they have no statutory-based standards to apply, only their judgments and knowledge to rely on. Unlike analysts in the domestic agencies, and in other national security agencies such as the Defense Department and CIA, their "data" are contained in imprints rather than print-outs.

Thus, at least since Dean Acheson's stewardship, most secretaries of state who wielded great influence (Dulles, Rusk under Johnson, and Kissinger) traveled light—in other words, without much departmental baggage. Strong secretaries often have been strong precisely because they ignored the department. When secretaries of state are perceived as representing departmental perspectives, they become especially vulnerable to competing sources of infuence—most particularly from within the White House. Why?

One must begin with the fact that foreign policy is a *high-priority* item. By its importance, its capacity to push other items on the agenda to a lesser place, foreign policy, though not equally appetizing to all presidents, becomes the main course of the presidential meal. The extent to which foreign policy is a focal point of attention, of course, depends on the extent to which any nation is deeply involved and committed as an actor in world affairs. And that, understandably, is related to a nation's capacities for such involvement.

Crises especially lend themselves to central direction. Foreign policy is often nothing but crises—either reacting to them or creating them. Filled with crisis and presumed to be of first-order importance, foreign affairs are, in fact, glamorous. Much more than apparently intractable or technically complex domestic problems, foreign affairs often seem to be contests of will—games against other players rather than games against nature.

In such games, regular bureaucrats are unlikely to be key players. Their instincts are to think small, to think incrementally, and to see the world in not highly manipulable terms. The glitter that presidents often see in foreign policy is at odds with the cautious instincts of the professional service entrusted to deal with it. Unattached to specific operational responsibilities and accessible to the White House, the NSC can take on the qualities of a think-tank unencumbered by the more limited visions that flow from the State Department itself. Moreover, the NSC, like any staff organization, is far more readily adapted to the changing foreign policy themes of presidents than a line bureaucracy such as the State Department. State is, of course, highly adaptable to modulated swings in policy, but not to strong oscillations. Organizational memory and bureaucratic inertia preclude it from reinventing the world every four years.

This particular difference in settings—White House staff versus line bureaucracy—also implies a difference in styles of policy analysis. One setting is accessible to power, the other more remote. One is especially attractive to the ambitious and purposive, the other to the cautious and balanced. One setting is tailored for "in-and-outers" and "high-fliers" borrowed from other agencies, while the other is meant for "long-haulers." One effect of this difference in settings is that even though the NSC staff is not overwhelmingly composed of academic figures, more NSC staffers are apt to be academics than their counterparts at state. For example, a study of senior foreign service officers indicates that fewer than 8 percent are Ph.D.s (Mennis, p. 71), while 58 percent of the NSC staff with which Zbigniew Brzezinski began were holders of the Ph.D. degree,[6] as are 43 percent of the present staff. Such differences do not reflect merely ephemeral circumstance. The

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[6] Compiled from data in Berry and Kyle (1977).

Reagan NSC also represents a mixture of scholars and government career officers with Washington experience (Smith, 1981). And as Destler describes the NSC staff under Presidents Kennedy and Nixon: "The typical staff members were not too different from the Kennedy period—relatively young, mobile, aggressive men, combining substantial background in the substance of foreign affairs with primary allegiance to the White House" (1972b, p. 123).

In other words, there is a correlation between background and organizational setting even though it is quite far from perfect. Backgrounds, of course, are only frail indicators of differences in syndromes of policy thought, and such differences need not imply substantive disagreement. Nonetheless, the correlation implies that the White House miniature of the State Department is more innovative than the real one at Foggy Bottom, more aggressive, and also more enthusiastic for White House policy directions.

The NSC staff and the national security assistant, of course, may conflict (as may the regulars in the State Department and their noncareer superiors). There have been notable clashes in the past, especially in the immediate aftermath of the American incursion into Cambodia in 1970 during the Vietnam engagement. The national security assistant and the NSC staff are not necessarily in agreement upon substance, but their *forma mentis* are likely to differ from those of their State Department counterparts. If presidents are served amorphous goo from the State Department bureaucracy (which they often see as representing other nations' interests to Washington), they may be provided with clear-headed principles from their in-house foreign policy advisers. Concerned with direction and results, presidents are usually predisposed to cut through the rigidities of complex bureaucratic systems and the cautions of the foreign policy regulars. In this, of course, lies the potential for isolating policy advice from implementation. Going through the bureaucracy often means spinning wheels, but ignoring the bureaucracy poses the prospect of personalizing policy rather than institutionalizing it. In this latter course, there is, to be sure, less wheel-spinning but there is, at least in the long run, also more spinning of castles in the air.

Finally, the soft technology of foreign relations means that it is just precisely the kind of thing that politicians think they are better qualified for than anyone else (Merton, 1968, p. 265). A former noncareer ambassador writes, for instance: "The average American has a sounder instinctive grasp of the basic dynamics of foreign policy than he does of domestic macro-economics. . . . Common sense—the sum of personal experience—will take one further in the realm of foreign policy than in macro-economics" (Silberman, 1979, pp. 879-80). Because little seems mystical or technical about foreign policy to presidents, reliance upon cumbersome bureaucratic machinery seems unnecessary. In most instances, presidents like to be directly engaged with foreign policy because it is more glamorous and central to their historical ambitions, less dependent upon congressional approval, and because it activates their "head of state" role (and in the event of possible military involvement, their "commander-in-chief" one also). In contrast to the trench warfare and haggling involved with domestic policy formulation, foreign policy making tends to promote self-esteem and presidential prestige. With all of these possibilities, it is improbable, therefore, that presidents want the powers of foreign policy making to be distant from them. Usually, they want it close to them. Presidents need to legitimate White House centrism, then, by investing it in a flexible staff operation headed by an unattached foreign policy "expert." These specific reasons, encapsulated within the more general determinants already discussed, have led the White House Department of State to loom as a contender for policy-making influence with the "cabinet" Department of State.

Irregular and Regular Syndromes

In spite of the alluring differences implied by the personnel distinction between irregulars and regulars, neither end of this distinction is a monolith nor is it always generalizable in the same ways across policy sectors and political systems. One reason for this is that bureaucratic cultures reflect the character of the host political culture, as those who have contrasted British and American administrative styles have observed (Sayre, 1964). Both British senior civil servants and their American counterparts, for instance, are bureaucratic regulars, yet they differ substantially in the manner in which they confront their roles—a difference that results from the political ambience surrounding them. Especially in administrative systems where there is little tradition of rotating officials across departments, there may be sharp differences in the characteristics of regulars across various departments. In the United States where these departmental subcultures are quite firmly implanted (McGregor, 1974, pp. 24-26; Seidman, 1980, pp. 133-73; Aberbach and Rockman, 1976, pp. 466-67), there is a stylistic gap between the entrepreneurial subcultures often found in social and regulatory agencies and the foreign service subculture of neutral competence.

Similar differences exist amongst irregular personnel as well, and as previously noted, American administration is permeated with irregulars.

American elite civil servants' responsibilities began at a level of authority significantly below that of their British counterparts. Indeed, as defined earlier, the distinction between "irregular" and "regular" within departmental settings *must be* hierarchically related. Still, the appointed irregulars in the departments often have had prior experience in that department, 35 to 40 percent according to one estimate (Stanley et al., 1967, p. 41). The professional perspectives of their departments often have been assimilated by these officials, and at least in this respect it is possible to distinguish them from the corps of presidential policy advisers.

Precise comparisons of administrative structures such as those of Britain and the United States are always perilous, but it would not be stretching matters excessively to say that Executive Office irregulars are somewhat akin to the high-fliers of the British civil service without the latter's attachments to the civil service system. Typically, they have less experience in government than their senior line counterparts. With the growth of the institutional presidency, however, the American system has displayed a penchant for mismatching titles of formal authority and possibilities for influencing policy. The high-fliers, therefore, often are better positioned to exert more policy influence in the American system than are the senior officials in the line departments. In the American system, proximity breeds possibilities.

Two very broad distinctions need to be made. One is that between personnel and their relation to career service channels. The other is between organizational settings—central staff versus line department. Further differentiations, of course, can be made within each of these categories. Table 1 illustrates the possible intersection of personnel career channels and their organizational settings. Although I have no measure of the relative influence of setting and career channel on policy thinking and behavior, it is likely that departmental appointees (cell B of Table 1) will be subject, with varying degrees of susceptibility, to the magnetic pull of their departments. Similarly, central staff officials with career backgrounds (cell C of Table 1), also with varying susceptibility, may be inclined to retain their career perspectives and be sensitive to their promotion opportunities—in short, to maximize their departmental interests even while serving in integrative staff structures. This problem reportedly plagued the NSC to some extent under Eisenhower (Falk, pp. 424-25). The distinction between cells A and D is obviously the purest. I assume here the probability of interactive effects between structure and personnel.

Beginning with a broad distinction between irregular staff and regular bureaucratic settings, Table 2 sketches some of the important respects in which these settings differ. These differences point to modal variations in function, in vantage point, in personnel, and in orientations to policy. In the analysis that follows, however, I start with differences between personnel, work back to settings, and then to forms of policy thinking.

How do irregulars differ from regulars? First, irregulars are more likely to be charged with coordinating functions (policy planning, for instance) than are regulars even when they are each engaged in departmental responsibilities. These functions provide the irregular with greater breadth and the capacity to see a more integrated policy picture, but one limited in depth. On the other hand, the regular is located so as to see detail but is less able or likely to see beyond it. These structural features also lead to different interpretations of rationality. The irregular is apt to define rationality as coherence from the vantage point of policy man-

Table 1. Career Channel and Organizational Setting Matrix

PURE

DIAGONAL

Career Channel: Irregular Setting: Central Staff A	Career Channel: Irregular Setting: Line Department B
Career Channel: Regular Setting: Central Staff C	Career Channel: Regular Setting: Line Department D

HYBRID

DIAGONAL

Source: Created by the author.

agement. The regular, however, is apt to see rationality in terms of informed policy making.

Free of operational responsibilities, irregulars are apt to be conceptualizing and deductive (more "theoretical" or "ideological") in policy thinking than are foreign policy regulars. Intimate detailed knowledge possessed by the regular tends to induce skepticism toward ideas that are abstract and aesthetically interesting. As the regular sees him, the irregular is a simplifier with tendencies toward an excess of imagination and a scarcity of discriminating judgment. Irregulars are rarely lacking in expertise; but their possibilities for thought are distanced from the immediacy of operational problems. Whether by role difference, by recruitment path, or by their interactive effects, the irregular is more disposed to theoretical thought than the regular. Theories are the precursors of activism for they simplify reality sufficiently to permit general, though not necessarily operational, plans of action. The inductivism that is more characteristic of the regulars leads them often toward perceiving complication; it leads them frequently to be skeptical about generalized schemes of action; often it leads them into paralysis. It is both the virtue and the liability of the regular's "hands-on" involvement that he will be predisposed to illustrate the invalidity of proposals and the assumptions they are based on than to advance alternative solutions. After all, it is normally the regular who has to live with the consequences of "rashness."

Ideas and skepticism, while polar intellectual traits, are nonetheless each valuable ones. Large bureaucracies are the wellspring of skepticism and the depressant of ideas. This bureaucratic characteristic flows from the inertia associated with established routines as well as from the concreteness of the regular official's world. Met daily, concreteness and detail induce awareness of complexity. It is this awareness of complexity that ironically is at the heart of the State Department's self-perception as a protector of real long-term interests (Gelb, 1980, p. 34).

Given their natural proclivities, regular bureaucrats are apt to be oriented to the long term within their specialized realms, and likely to be skeptical of overarching themes. This characteristic is not especially attractive to presidents whose "common-sense" approaches to foreign policy often coincide with what is also politically supportable. Being policy generalists, presidents tend to be impatient with "can't-doers," failing to understand or appreciate the skepticism of the foreign

Table 2. Differences between Irregular Central Staff Settings and Regular Bureaucratic Settings in Foreign Policy Making

	Irregular Staff Settings	Regular Bureaucratic Settings
Typical Responsibilities	Coordinating functions which provide breadth and integrative perspectives, and foster coherence	Implementing functions which provide detailed knowledge and particularistic perspectives, and foster local rationality
Location Relative to Decisional Authority	Proximate to political authority, therefore perceived as "Us"	Distant from political authority, therefore perceived as "Them"
Type of Personnel	Irregulars and regular "floaters" with few organizational commitments	A mix of irregulars and regulars toward the top, with regulars with long-term organizational commitments at the core
Typical Policy-Making Styles	Activists Theorists Conceptualizers Deductivists "Simplifiers"	Skeptics Specialists Inductivists "Complexifiers"
Dominant Policy Implications	Directive and thematic, initiatory and bold	Cautious and nonthematic, incrementalist and narrow-gauged
Resulting Policy Problems	Superficiality	Particularism

Source: Created by the author.

policy regulars. From the presidential vantage point, sober thoughts are mere fudge, and skepticism rarely accords with presidents' political needs. Unattached foreign policy "experts," on the other hand, can articulate ideas and push proposals unencumbered by bureaucratic constraints or operational responsibilities. This gives them an obvious advantage over those representing the particularizers in the foreign policy bureaucracy. As for the secretary of state, his advisory and policy-making roles will likely be as large as his distance from the department is great.

There are dangers in the detachment of policy advice and policy influence from operational responsibilities. The triumph of theory over fact is obviously troubling. If regulars, by their skepticism (and probably also their convenience) tend toward incrementalist thinking, it is also true that, at least in the short run, no one ever died of incrementalism. Still, the failure to produce and institutionalize policy integration can be a long-term carcinogenic agent. For politics contoured only by those with operational attachments are likely to suffer from deficiencies of imagination.

Proposed Solutions to the Integration Problem

The problems of generating integrated and informed policy are obviously apt to receive attention in inverse proportion to the power of the political tools for achieving it. By this standard, America's foreign policy machinery is beset with continuing difficulties. Proposals, official and unofficial, to remedy the foreign policy machinery of the United States abound. They tend to fall into three broad classifications: (1) those emphasizing the role of a strong State Department with a powerful secretarial and presidential direction; (2) those emphasizing the importance of multiple streams of information with a national security assistant playing the more traditional role of traffic manager rather than the one of advocate acquired over the last two decades; and (3) those emphasizing strengthened cabinet-level coordination and the interchange of officials beneath this level. My intent here is to highlight their particular perspectives and their uncertainties.

1. Strengthening the Secretary of State. This is not only a common proposal, but also one that seems most obviously apt to connect political strength to institutional capabilities. As Destler has put it:

> The issue is not whether the Secretary or the President has primacy. Rather it is who—the Secretary or the National Security Assistant—should be the central foreign affairs official short of the President and acting as his "agent of coordination." If the President is known to rely primarily on the Secretary of State for leadership in foreign policy-making across the board, he should prove far more formidable than a "mere cabinet officer" (1972b, p. 359).

A strengthened secretary of state, however, must have the confidence of the president, and this, in turn, requires a strengthened State Department which means, in Destler's view, a lessened diplomatic role for the secretary and a more forceful policy advocate and organizational management role. What Destler has in mind by the latter, however, is essentially a State Department so transformed that it would be a more coherent tool of presidential direction. In other words, an important element of Destler's proposed reforms is to do unto the State Department that which often has been tried in domestic departments: politicize it. Again, in his words:

> There will remain an inevitable tension between the interests and predispositions of Foreign Service officers and those of Presidents. So no Secretary of State who did not build a strong "political" component into the State Department could hope to satisfy a President bent on controlling the foreign affairs bureaucracy (1972b, p. 288).

Although it does not do full justice to Destler's arguments to say that coherence from the president's standpoint is the exclusive value with which he is concerned, his prescriptions move in the direction of making it the primary one. The potential trap, as Alexander George has noted, is that managerial rationality would come to displace substantive rationality, a likely probability if the State Department is to be politicized, if in essence it is to become a larger, deeper NSC (1972b, pp. 2811-83).

In this guise, a strengthened secretary of state necessitates a weaker national security assistant, indeed, a virtual elimination of the position. A strong secretary of state with a close relationship to the chief executive, Destler claims, has been the best check on the role of the national security assistant as a central policy advocate. But, as he also notes, the very existence of the assistant in the White House makes it difficult to generate that close relationship (1980, pp. 86-87).

The responsiveness of the State Department, of course, is also dependent upon a president knowing his own mind. Presidents differ in this regard, but it is not immediately clear how consistent they can be concerning policy directions to what, after all, are mostly reactive opportunities and necessities. Thematic agreement may be conducive to operational agreement, but it can be no more than that. Alternatively, overarching clarity in foreign policy may simply be dogmatism.

A more likely possibility, one that may be symbolized by personalities such as Alexander Haig and Henry Kissinger, is that of the entrepreneurial secretary of state who cuts a demanding figure in his own right. The entrepreneurship, however, may well come at the cost of organizational debilitation. While the relationship between Alexander Haig, the State Department, and the White House remains to be developed as of this writing, Kissinger, as secretary of state, was both a policy advocate and presidential spokesman, but in spirit he never left the White House in these roles. Nor, in fact, had he physically left it until late 1975. A secretary who draws nourishment from the Department's professional foreign service roots, however, is apt to find himself, sooner or later, designated as one of "them." This, at least since Dean Acheson, has largely been the case.

In any event, the problem lingers of generating political coherence (organizational rationality) in such manner as to effectively utilize substantive rationality (derived from specialized sub-units). A politicized State Department, one suspects, would be more coherent and responsive. But could it then effectively contribute to informed policy making?

2. **Encouraging Multiple Advocacy.** As proposed by Alexander George (1972a), the organizational strategy of multiple advocacy assumes the virtues of local rationality. In spirit, it is to foreign policy formulation an application of Chairman Mao's "Hundred Flowers Bloom" campaign. It makes a virtue of what is a necessary vice, the multiplicity of perspectives generated by the division of labor which bureaucratic sprawl leaves in its wake. George's interesting suggestion is to return to the basic concept of the old special assistant's role as a managerial custodian, a facilitator of varying perspectives so that the president may avail himself of the full play of diversity surrounding him. In its new form, the assistant would be constrained from playing the role of policy advocate, or from presenting foreign policy views to the public. His facilitating role would be greatly expanded, and in that presumably would lie his status. To some extent, this super-custodian presumably would be something akin to the director of the Office of Management and Budget, but without the capacity to pass judgment upon departmental requests—in other words, largely powerless. I am not the first to point out that in Washington those with status but without power quickly become worked around rather than through.

We may question, too, the assumption that presidents, or leaders of other large organizations, for that matter, are thirsting for information, diversity, and knowledge. Mao, after all, quickly came to disown the "Hundred Flowers Bloom" campaign. Facts, information, knowledge are great legitimizers of action. Not surprisingly, leaders often find it best to screen them selectively. A reasonable hypothesis is that the longer presidents (leaders in general) have been in power and thus the more prior commitments they have established and defenses they have constructed around them, the more likely it is that their tolerance for diversity declines. Even if at first presidents are predisposed to hunt facts, in the end facts are more likely to haunt presidents. Removed from electoral concerns, presidential interests in policy, per se, are often suspect (recall Nixon's response to Haldeman's request for policy direction on propping up the Italian lira), but whatever interests they do have also are apt to diminish as their term wears on and as more decisions become responses rather than initiatives. None of this perhaps would be so important were it not for the fact that George insists that presidents must assume a magistrate's role; otherwise, diversity becomes hyperpluralist babble. For without this particular role assumption, the rich flow of information and analysis likely would reinforce local rationality. Coherence and direction would famish.

The assumption that foreign policy contestants ought essentially to emulate lawyers in an American litigation proceeding by pressing their "interested" rendition of the "facts" before a disinterested presidential magistrate is curious. Being that presidents are neither disinterested themselves, nor unlimited in their attention spans, it is more likely that the chief magistrate will be the president's national security assistant. There is, in short, little incentive for the president to cope with detailed arguments, and none for the assistant to shy away from policy advocacy. As interesting as they are, proposals for functional changes that do not account for the costs to, and incentives of, the actors involved are more nearly prayers.

3. **Strengthening the Cabinet and Rotating Officials.** In *Presidential Power,* Richard Neustadt quotes a White House aide to President Eisenhower as saying, "If some of these Cabinet members would just take time out to stop and ask themselves, 'What would I want if I were President?', they wouldn't give him all the trouble he's been having" (1980, p. 31). The reasons for this estrangement are well known. And the underlying assumption about it, namely, that role alteration diminishes parochialness, is taken as the point of origin for its alleviation. To alleviate this condition, a two-pronged strategy has been advanced (Allison and Szanton, 1976, pp. 78-80; Allison, 1980). The strategy requires a dash of something a bit new and something old.

What is somewhat new is the recommendation that NSC staffers be continually rotated between the agencies and the White House so as to mold together agency and White House perspectives among individuals. The model for this suggestion is that of the British civil service generalist. As matters stand, of course, a substantial portion of the NSC staff previously served in another agency (State, Defense, and CIA, in that order) either indirectly or directly before arriving at the NSC. Estimates from the 1977 list show that nearly 40 percent had such experience (as compiled Berry and Kyle, 1977), and to define things more narrowly, Destler (1972b, p. 249) indicates that as of April 1971 almost half of the NSC staff had some prior experience either in the State Department or the military services. Thus it is not that many of these officials are lacking experience in the agencies (though it should be kept in mind that over 60 percent of the 1977 staff had no prior agency experience), but rather that for most of them, present roles are likely to be especially compelling.[7] Recombinant socialization does not necessarily mean intellectual integration. In the face of a highly centrifugal structure of government, knowing how it looks from "there" may be merely a tactical advantage in the struggle to influence policy rather than a basis for policy integration. For such proposals to work, structures that provide for collectively responsible points of decision making are essential.

Thus, the other part of this recommended strategy is to create an Executive Committee of Cabinet Officials (ExCab) to provide ongoing high-level policy review. "A body like ExCab," Graham Allison claims, "would yield most of the advantages of the collegial participation of major department heads while avoiding the unwieldiness of the full cabinet" (1980, p. 46). ExCab, however, as even its promoter willingly admits, is not an altogether new idea. The Nixon administration after all, had proposed a set of "super cabinet" departments and, failing congressional approval, then created by executive fiat an informal set of "super-secretaries." Though Nixon's political demise brought this operation into formal disrepair, it is unfair to pass judgment upon it, since its creation only shortly preceded Nixon's calamitous, if protracted, fall from grace. To be sure, there are a number of operational problems that this approach does not automatically avoid. There is first the question of who is in and who is out. Only in a cabinet of nonentities of the sort Nixon tried to create for his second administration is it likely that department secretaries would accede to more powerful presences. Secondly, while the ExCab proposal potentially permits diversity to flow with decisional responsibility, many of the difficulties presidents have in dealing with the full cabinet also arise even with a reduced foreign policy-focused cabinet. It is not merely the presence of diversity within the executive that distresses presidents, for that evidently is a condition affecting top leaders everywhere to some degree. Rather, it is the ease with which opposition or losing forces within the executive can go to Capitol Hill or the press, usually reaching the former by means of the latter. It is difficult for collegial government, however reduced the number of relevant actors, to flourish under such conditions. Any proposal to reform the organizational apparatus of American foreign policy making needs to be sensitive to this problem. Though the possible, but as yet unknown, impact of organizational reforms should not be disconnected, these neither alter fundamentally the institutional framework of largely antagonistic forces in Washington nor the culture of openness that both sustains and reinforces this adversarial framework.

Conclusion

Presidents ultimately determine foreign policy. Whatever system of advice and decision making exists can exist only with the president's approval. It is within the range of presidential discretion to permit the national security assistant to become a leading contestant for foreign policy influence. Similarly, it is within the scope of presidential judgment to permit the national security assistant to appear as the chief foreign policy representative for their administrations. Nixon and Carter did permit these things; indeed, they encouraged them, though for different reasons. Thus far, the Reagan model (if there is one) has resulted in decreased visibility for the national security assistant. The NSC professional staff, however, is no smaller than it was during the Carter administration, and at least one report indicates a more direct White House staff involvement monitoring operations through the NSC machinery (Evans and Novak, 1981). Additionally, somewhat reminiscent of Nixon's "administrative presidency" model, a loyal operative has been slipped into the deputy secretary's role at State. In the last 14 months of the Ford administration, the role of the national security assistant and, to a modest degree, that of the NSC veered closer to the Eisenhower model of a dominant secretary of state and a "neutral competent" national security assistant (Brent Scowcroft). The reason for this, however, now seems clear. Ford's secretary of state, Henry Kissinger, was his leading foreign policy spokesman and leading foreign policy maker, yet not really his foreign minister. To be both, foreign minister (representing departmental perspectives) and leading foreign policy maker has within it increasingly the seeds of an insoluble role conflict.

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[7] The Carter NSC figures are essentially reversed under Reagan. Among the present NSC staff, roughly 60 percent have had prior government experience, and 40 percent have not.

Presidents vary, of course, in their ideas as to how foreign policy making ought to be organized, what they want from it, and how much weight is given at least at the outset to the values of harmony and diversity. The difficulty lies in isolating which aspects of their variability will lead to a heightened emphasis upon staff irregulars, and how they will be used. Similar results, as the disparate cases of Nixon and Carter indicate, may flow from different organizational modes. While each held widely different models of the policy-shaping process in foreign affairs, each also further enlarged the role of the NSC as a policy mechanism. Early on, Nixon seemed to prefer policy to be shaped at the White House, and as much as possible to skirt around the bureaucracy. Carter's organization, on the other hand, seemed to exaggerate Alexander George's ideal of multiple advocacy except, quite importantly, that Zbigniew Brzezinski was meant to be an advocate and not just a mediator. Different intentions seem to have produced fairly similar results—a highly visible national security assistant and a "competing" State Department.

The variability of presidents notwithstanding, the overall thrust since Eisenhower seems fairly clear: more White House centrism in foreign policy making, and an enlarged NSC role. Presidential variability tells us a lot about form—the particular uses made of the NSC mechanism and of the national security assistant—but it does not tell us why the NSC today looks so different from the NSC of 25 years ago, nor does it tell us why the national security assistant has so often been a primary policy maker. While the water has both risen and receded, the watermark is a good bit higher now than it was then.

To explain this trend toward centrism, and thus the importance of policy irregulars, my analysis focuses upon a theory of government—a theme somewhat broader than its specific target. The proposals for reconstituting America's foreign policy mechanisms that have been examined here certainly represent a more precise approach. Yet, government and politics in Washington, and the open culture that surrounds it, represent the limits against which these various proposals bump. From the hyperpoliticized ambience of American government the role needs of foreign policy contestants are shaped. Institutional fragmentation and weak parties not only beget one another, they also promote a level of bureaucratic politics of unusual intensity—grist for the mill of a highly inquisitive press.

No wonder presidents find their political and policy needs better served from within the White House. From this vantage point, the departments sooner or later are perceived as representing or pursuing interests that are not those of the president. This is especially so for the State Department because it is frequently seen as representing interests of other countries. With virtually no domestic constituencies and reflecting a subculture that, much like the British civil service, emphasizes "neutral competence" and balance, the foreign service regulars in the State Department are singularly disadvantaged. The steamy adversarial climate of Washington's executive politics does not nourish such values. The White House (and often department heads) are anxious for "movement," and unreceptive to "let's wait a moment." In the long run, the danger in any such setting is that the tools of central clearance will metamorphose into mechanisms for central dominance.

In sum, the reasons why America has a competing State Department turn out to be both excruciatingly complex and yet remarkably simple. Its simplicity lies in the structure of antagonistic forces given form by the American Constitution. Its complexities lie in the conditions—the importance of foreign policy, the role of the media, the burgeoning of policy intellectuals—that have since ripened.

The problem of reconciling "the persistent dilemmas of unity and diversity" (Fenno, p. 339) remains to be solved as much in the foreign policy sphere as in the domestic one, especially as the distinction between these arenas erodes. In unity lies strategic direction and clarity, but also the dangers of a monocled vision. In diversity lies sensitivity to implementation and to nuance, but also the dangers of producing least common denominators. Ironically, during the Eisenhower presidency when the NSC performed most nearly like a cabinet committee producing consensus from diversity, it was criticized for the ambiguities remaining in its products (Destler, 1977, pp. 152-53). If not a fudge factory, it was at least a fudge shop.

Each president to some extent will develop mechanisms that suit him best. Among other things, the policy system established will reflect the idiosyncracies of interpersonal chemistry. Each, though, has inherited an in-house foreign policy apparatus defined in the last 20 years more by how it has been used than by its original statutory rationale. How that apparatus will evolve cannot be foretold with preciseness. But how and why it has evolved from its inception to its present state is a saga that should be of as much interest to students of American government as to those of foreign policy.

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Political Commentary on the History of Political Theory

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The phrasing of the title telegraphs the thesis of this essay: study of the history of political theory ought to be understood and approached as a branch of political studies. Prior to the postwar success of empirical political science, the view would have seemed unexceptional. But the need for a defense against the empiricist attack impelled many theorists to turn to theories of interpretation in search of a philosophical and methodological identity. A preoccupation with issues of interpretation now threatens the customary, political study of the tradition. This essay, written in defense of a political understanding of the field, identifies fundamental propositions distinguishing "political commentary" and suggests critical standards appropriate to the enterprise. It also criticizes two currently fashionable applications of interpretative theory to the study of politics, historicist commentary and hermeneutical political science.

Commentary on the history of political theory is a mode of political discourse. Prior to the postwar success of empirical political science, this claim would have seemed unexceptional, albeit contested by some historians and philosophers (Gunnell, 1979, pp. 12-22). But in the last decade it has received a new and more subtle challenge, a challenge potentially more debilitating precisely because it has emerged from within the field of political theory. To respond to the criticisms of the empiricists, some theorists discovered a useful ally in that tradition of the human sciences which conceives of knowledge of human things as essentially and necessarily interpretative in character (see Bernstein, 1978; Dallmayr and McCarthy, 1977; Connerton, 1976). The alliance provided theorists with the means to critique positivism in the social sciences, thereby restoring the study of political theory to a position of some security within the political science discipline. But the restoration generated its own costs. It led to the substitution of a concern with issues in the philosophy and methodology of interpretation for a focus on political issues.[1] In short, political theory's alliance with the human sciences threatens to depoliticize the study of the history of political theory and to transform it into a branch of interpretative inquiry, more akin to literary criticism than to political argument.

The present essay is written in defense of a political understanding of commentary on the tradition. It is organized into two parts, an argument for the idea of "political commentary" followed by a critique of interpretative approaches to political theory. The idea of political commentary (which is a shorthand way of identifying my position) is intended to give theoretical representation to a customary kind of practice. Despite current theoretical debates over interpretation, there are numerous theorists, located in departments of politics, who continue to think of themselves as political thinkers just because they study the history of political thought. Any deliberately political treatment of the tradition would qualify as an example of their practice, McGovern's *From Luther to Hitler: The History of Fascist-Nazi Political Philosophy* (1941) as much as Macpherson's classic *Political Theory of Possessive Individualism* (1964). Works that would not qualify as political commentaries are, at one extreme, those in which the tradition figures only as a source for name-dropping and, at the other, those in which political concerns are secondary to exegetical ones. In so demarcating a field of practice, I am not suggesting that the tawdry and the classic are of equal stature. Once I have established the fundamental propositions that define political commentary and distinguish it from exegetical commentary, my point is then to develop critical principles specifically appropriate to political commentary. However, the fundamental propositions themselves ought to be acceptable to any commentator whose work is intentionally political.

Moreover, to a point, the propositions apply to all commentary relating to politics, and so form the basis for the critique in the second section of the programmatic challenges to traditional practice currently issuing from philosophers of interpretation. Historicists like Skinner (1969) and Dunn (1968), among others, would render the history of political theory but a branch of the history of

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[1] Kress (1979) argues along similar lines. It is ironic, but revealing, to recall that but a decade ago Wolin (1969) was taking empirical political scientists to task for their focus on methodology. Also, see Feyerabend (1978) for a most incisive and provocative critique of methodological thinking in the philosophy of science.

ideas, a move attacking the political importance of the field. Hermeneuticists such as Habermas (1970a, 1970b) and Ricoeur (1971) pursue the idea of a "human," i.e., interpretative, political science, and so call into question the importance of studying the great theories of the past. The idea of political commentary is specifically directed against these approaches to political theory.

In the interest of clarity, the related terms "interpretation," "exegesis" and "commentary" will be used in specialized ways in the discussion. "Interpretation" and "interpretative inquiry" connote the enterprise of understanding phenomenal meaning and significance, usually textual meaning and significance. "Exegesis" refers narrowly to the explication of textual meaning. "Commentary" refers to writing about the history of political theory, regardless of orientation.

Political Commentary

The idea of political commentary is founded on two propositions: first, that commentary is an autonomous, substantive mode of discourse, not simply a derivative report on the writing of others; and second, that commentary on political theory is a species of political writing. Essential to the first proposition is a refusal to grant an inherent difference between first-order inquiry, the "original" texts, and second-order inquiry, i.e., commentary. The fact that commentary is by definition discussion of other texts, and takes as its special problem the task of understanding their meaning, distinguishes the genre. But the distinction does not go to fundamentals: commentaries, through the medium of interpretation, make their own arguments. The choice of texts for interpretation, the approach taken to interpretation, the interpretative argument—all these compose the "first-order" argument of commentary. To put it concisely, texts and textual meaning are the characteristic material and problematic of commentary as first-order discourse.

The second proposition is that commentary on political theory is a mode of political discourse. The refusal to grant an inherent difference between first- and second-order inquiry implies, more specifically, a refusal to grant an absolute distinction between the tradition of political theory and commentary on that tradition. Admittedly, the growth of academic commentary in this century has coincided with the decline of theorizing in the grand, traditional style. This coincidence is itself a topic for reflection. Still, what has not declined is a concern with the political themes that preoccupied the theorists of the tradition. Commentary certainly differs from grand theory, and it is certainly valid to question whether commentary is capable of continuing the tradition of political thought which is its subject. But to assume a hard line between first- and second-order thought in the field would be to concede at the outset a qualitative difference between our aspirations as political thinkers and the achievements of the great thinkers of the tradition.

Neither proposition is self-evident. Historians and philosophers, especially, have rival views and claims regarding the history of political theory. For historians, this literature is part of the history of ideas. Certain philosophers, on the other hand, consider the history of political theory to be essentially philosophic in character, a literature of philosophical reflection on political topics, and therefore their special bailiwick. Some philosophers and almost all historians would quarrel with the proposition that their discussions of the history of political theory are similar to the originals (for example, Quinton, 1967, p. 3; Pocock, 1980, p. 566), not to mention the proposition that commentary is political discourse. The easiest response to both the historian and the philosopher would be simply to note the vast range of materials and problems meriting attention, to praise once again an intellectual division of labor, and to encourage political theorists engaged in political commentary to proceed doing what they do best. Unfortunately, this easy response is not really available; it glosses over most of the important issues surrounding the case for political commentary. To defend political commentary on the history of political theory as a form of first-order discourse and as a type of political writing necessarily entails a series of claims about the proper norms or considerations that should control how one does political theory and, more particularly, how one should approach the history of political theory. In short, the case for political commentary cannot avoid implying that the political theorist *qua* political commentator adopts the most satisfactory relationship towards the history of political theory.

There are, I think, two major grounds supporting the political theorist's claim: analytic approach and orientation towards the world. Even those philosophers who claim a similarity between their enterprise and the tradition nonetheless go about political philosophizing by applying philosophical analytic approaches to political topics and texts. Since these approaches—for example, ordinary-language analysis and conceptual analysis—derive from philosophical sources and have broader application to many fields of inquiry, they carry the risk of transforming political problems into philosophical puzzles. "Sometimes," Pitkin suggests in *Wittgenstein and Justice,* political theory and philosophy "will be useful to each other or even overlap; sometimes they will be far apart, and conceptual or philosophical problems may get in the way of work in political theory" (Pitkin, 1972, pp. 300-01). *Anarchy, State and Utopia* has been diagnosed by Barber as precisely the last sort of work, a case of philosophical "deconstitution" of political theory:

> Nozick believes that he is propounding a logically coherent argument in defense of pure individual freedom. And so he is; and, in philosophical terms, with considerable success. But a logically coherent argument by philosophical standards is not yet a political argument at all. Philosophical argument offers abstract alternatives; if none seem defensible, none are accepted; we simply return to point zero. Political argument begins with concrete givens; if abstract alternatives prove indefensible we do not return to point zero; we are left rather with the givens. The relevant political question therefore becomes: is a particular alternative *more* tolerable/acceptable/legitimate than the given to which it is a response? If so, regardless of its status as measured by abstract and absolute philosophical criteria, it is the preferable alternative (Barber, 1977, pp. 20-21).

Political theory, like philosophy, possesses analytic schools, Straussians versus Arendtians and so forth. Unlike their philosophical equivalents, however, these schools derive their analytic approaches from reflection, specifically, on the nature and purpose of political life.

Closely related to the distinction between philosophical and political approaches to political theory is the contrast between contemplative and practical orientations toward the world. "Let the truth be told though the world may perish"—and its practical corollary, "If reality cannot be justified, all the worse for reality"—express attitudes essential in philosophical and historical inquiry.[2] The goal of these two sorts of truth-telling is understanding the world; and if understanding turns out to be dangerous, this creates a problem, but not a ground for forsaking truth. However much political theorists may value contemplative understanding, our enterprise issues from a different orientation, from the sure knowledge that reality will not go away. The world presses problems on us; we are not, at the start, curious about it (cf. Pitkin, 1972, p. 300).

Commentators on the history of political theory can claim to be the rightful heirs of the tradition because—and only to the extent that—our inquiries respond to problems in the world. The idea of political commentary reflects this orientation. It emphasizes the active side of commentaries as statements in their own right, with impact in the world, in preference to an exegetical (contemplative) emphasis on the task of understanding another's meaning. So understood, political commentary and political theory are defined by their engagement in the world, the world being also their object of study. We will later have to ask how study of the history of political theory can be a practical response to pressing worldly problems. But the example of the great theories bears remembering: political thinking can be practical without being merely topical or policy-oriented. A practical definition of commentary on the history of political theory is not the only sensible view; the contemplative stance has its claims, too. But it is *a* sensible view; and it is entirely preferable, I will argue in the second half of this essay, to the historicist and hermeneutic versions of the contemplative position that transform political theory into simply a branch of interpretative inquiry.

Commentary on political theory is a substantive mode of discourse in its own right, and it is, specifically, a mode of political discourse. A third, prescriptive assertion completes the framework of introductory propositions: commentary ought to be deliberately and transparently political in character. This prescription contrasts with the central characteristic of interpretative theories: a fundamental interest in understanding meaning and arriving at the truth about a text. On one level, it seems quite straightforward to say that, being motivated by political interests, commentaries ought to display those interests for public scrutiny. But from another angle, the assertion is the most controversial of all because it appears to endorse the introduction of bias into commentary. It immediately brings to mind the alternatives of attentive, accurate, careful exegesis versus the biased and manipulative use of textual material to make a political argument. Who could but prefer the former? This reaction is at the root of suspicions about political commentary, and is the foundation of the appeal of interpretative theories.

The reaction rests, however, on the unspoken assumption that there are standards governing exegetical commentary, whereas political commentary responds only to the goal of persuasive argument. At least with exegetical commentary, there are a wealth of critical norms. Accept the idea of political commentary, and it looks as though indifferent toleration must be the order of the day. In short, if political commentary is to acquire the status of an autonomous mode of discourse, no longer hostage to some outside interpretative yardstick, critical standards are required appropriate to the genre. It is not a question of substituting one set of critical norms, "political" norms, for another set, "exegetical" norms. Of course, standard exegetical norms of scholarly accuracy apply to political commentary. However, exegetical norms do not exhaust the universe of standards appropriate to commentary on political theory. To the contrary, "political" norms ought to be seen as the more important of the two, since they relate to the substance of the activity. Whether political and exegetical standards conflict is an issue that should not be treated in the abstract, but only in the context of discussion of specific norms.

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[2] The first phase is a loose translation, though faithful to her meaning, of an Arendtian phrase, "*Fiat veritas, et pereat mundus*" (Arendt, 1968b, pp. 227 n., 228, 260). The second telescopes a point in Barber's critique of Nozick (1977, p. 21): "To the philosopher, what cannot be justified, cannot be. If reality defies the prohibition, all the worse for reality."

Before considering the criteria appropriate to political commentary, we need to say something about its subject matter: the history or "tradition" of political theory. Political commentators' use of the idea of a tradition is currently a matter of some controversy. For example, Gunnell properly criticizes the transformation of the idea into a myth, a transformation which has essentially come about by "bestowing misplaced historical concreteness on what was originally an ideal typification" (Gunnell, 1979, p. 87). The idea of a tradition of political theory is an abstraction, partly based on customary practice and partly a matter of interpretative definition (Pocock, 1962, pp. 183-84, 186-87). As an abstraction *from* historical practice, the criterion appropriate to specific identifications of the tradition, and strands within it, is empirical, historical accuracy (Pocock, 1962, p. 188). But the abstraction has another side; it also forms part *of* the first-order argument of a political commentary. As such, characterizations of the tradition may be criticized for the view of politics they convey. Thus, to illustrate, it would be legitimate to evaluate Leo Strauss's schematic criticism of the modern tradition in terms either of historical accuracy or political theory; and the latter sort of critical evaluation is the more crucial to commentary considered as political writing.

All this is only to suggest the terms on which commentators may apply the idea of tradition to the history of political theory. But I think there is the stronger claim to be made that political commentators need the idea of a tradition. Assuming political theorists are first and foremost political thinkers, theorists who pursue commentary are theorists who have chosen to think about politics by way of studying the history of political theory. If the choice is to have more than an arbitrary, academic rationale, it must hinge on some estimation of the unique character of this body of literature. Practical orientation is one such characteristic, to which may be added, second, the level of questioning characteristic of the classic texts, their concern with fundamental issues of the nature and limits of political life, and third, as will be discussed later, the comprehensive scope that is commonly characteristic of the classics. These characteristics add up to the assertion that there is a more or less definable tradition, identifiable as a simultaneously practical, theoretical and comprehensive kind of political inquiry. Admittedly, texts in the tradition vary on these three scores; nonetheless, rough, common-sense discriminations among texts that do and do not belong to the tradition are not especially difficult to make. Political theorists need this tradition as our source for learning how to think about politics in this distinctive manner. The purpose and structure of the classic texts are like a sextant for theorists, helping them to locate and define their own activity in commenting on those texts. Philosophers and historians learn their activity from exemplary texts. Entirely similarly, texts in the tradition, together with good political commentaries, are political theorists' exemplars. Grant the view that contemporary political theorists ought to emulate the masters of the past, and it both follows that we apprentice with the masters when we study the history of political theory and that we need the idea of an exemplary tradition to guide our apprenticeship.

Criteria of Political Commentary

Now let us turn to the criteria generally appropriate to political commentary, whether or not such commentary employs the idea of tradition. At least three sorts of criteria apply, each relating to a different dimension of political commentary. Insofar as commentaries are a species of *action,* moral judgment is appropriate. As, more particularly, instances of *political action,* political judgments apply regarding both content and method. In this sphere, judgments are likely to be consequentialist or interested, but criticism is none the less rational for this being so. Finally, as an *interpretative* mode of political discourse, commentary is required to be truthful. This last is the area in which exegetical criticism enters the picture. Such criticism is, however, only one application of the injunction of truth-telling.

I want to begin with the connection between moral and interpretative (or, more generally, philosophical) judgment. Responsibility and veracity are elementary principles in these spheres, and they are related principles in the very straightforward sense that truth-telling is an ordinary part of our moral responsibility for our conduct.[3] Applied to commentary, this ordinary awareness provides a useful way of unraveling some of the mystery of the relationship between second- and first-order dimensions of the activity. Even narrow exegesis, which appears entirely devoted to the truth of a text, in fact rests on a moral claim: the claim that telling the truth about a text is *the* moral responsibility characterizing the comentator's enterprise (Hirsch, 1972, pp. 259-61). There are, surely, a wider variety of ways of conceiving commentators' responsibilities.

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[3] An excellent discussion of the connection between responsibility and veracity appears in Tinder (1976, pp. 62-70).

Texts are not the only reality to which commentaries refer. The world in which a commentary is set is equally a reality about which the commentator has a responsibility to tell the truth, to the best of his or her ability. Given that discussion of texts is the distinguishing feature of commentary, embracing this responsibility requires reflection on the significance of interpretative projects in their concrete particularity, i.e., responsible commentary requires reflection on what it means to interpret these certain texts, in this way, and toward these conclusions, *in this context.*

The controversy surrounding Arendt's *Eichmann in Jerusalem* provides an illustrative interpretative analogy. It is an especially interesting illustration because the controversy pits moral responsibility towards others against the responsibility of telling the truth. Gershom Scholem, for one, found Arendt wanting on the former score (1978, pp. 241-42):

> A discussion such as is attempted in your book would seem to me to require . . . the most old-fashioned, the most circumspect, the most exacting treatment possible—precisely because of the feelings aroused by this matter, this matter of the destruction of one-third of our people.

Arendt's reply to Scholem emphasizes the duty of truth-telling, and the danger of compassion as a motive for concealment of factual truths (Arendt, 1978, pp. 247-48). The dispute teaches two lessons. First, moral evaluation of interpretative statements encompasses their contextual significance, as well as their empirical (textual) accuracy. And secondly, recognition of this dimension of interpretative responsibility of course spells an increase in the difficulty of the commentator's task.

Admittedly, commentaries on the history of political theory do not have the kind of political impact of a work like *Eichmann in Jerusalem.* Doesn't this difference mitigate political commentators' worldly moral responsibilities? Perhaps the exegetical commentator's narrow commitment to explicating textual truths derives, in fact, from an accurate perception of the relative worldly irrelevance of the activity. Certainly, commentaries vary widely in their practical significance. Some ideas and some texts are of only historical interest; commentary on them has become a matter of explaining archaic themes. Excluding this largely scholarly activity (which nevertheless can be diagnosed as reflecting an [anti-] political position), practical relevance can be claimed for commentaries on the tradition if one bears carefully in mind their context and audience. In view of the academic orientation of most contemporary commentators, the two main vehicles of their influence are education and public discussion within the somewhat larger intellectual world. (It is important to recognize that this is a matter of circumstance, not of characteristics inherent to the enterprise. There have been times when historical scholarship has been quite central to immediate political debate, for instance, seventeenth-century England, as Pocock has detailed in his *The Ancient Constitution and The Feudal Law* [1967, Ch. 8] and there is even today the example of the Marxist tradition in which commentary constitutes a vehicle of political analysis and argument [see, for example, Anderson, 1976]). Nonetheless, because contemporary commentary is read for the most part by teachers, students and intellectuals, its influence is admittedly felt at an elite level. But surely Arendt's nostalgia for the *polis,* Strauss's critique of modernity, Macpherson's dissection of liberalism, to name several of the obvious examples, have shaped and articulated critical responses to contemporary American politics. What is special, and specially important, about these commentators' practical impact has to do with the kinds of questions they bring to public attention: questions concerning the fundamental assumptions of our political life and the legitimacy of our institutions. Again, insofar as commentators are the rightful heirs of the tradition, this is precisely what commentators ought to be doing.

Granting that political commentaries indeed have practical impact, one observes in the controversy over *Eichmann in Jerusalem* an exemplary lesson: commentators' responsibilities go beyond the narrow responsibilities of the exegete to encompass, as well, the much broader responsibilities of political actors. It matters what sort of understanding of the world commentaries convey to their readers. Thus, in turn, commentaries are legitimately criticized for their worldly import.

The range of possible criticisms extends from disagreement to condemnation. One can disagree with the statement about the world conveyed by an interpretation without holding the statement to be iniquitous or patently false. Take, for instance, Michel Foucault's treatment of historical eras as discontinuous strata, a vision that denies progress and so deprives us of one way of making sense of the world. It is entirely reasonable to grant Foucault the cogency and persuasiveness of his argument and yet still to disagree with it, always of course with reasons attached.

Iniquity and falsehood are the stronger versions of the same critical sensibility. Perhaps the most usual form of such criticism involves analysis of the implications of an interpretative project. The commentator is granted the intentions of veracity and responsibility; the charge is that he or she has failed to see how the project is iniquitous or has failed to see the falsehood involved. This kind of criticism is illustrated in the following remarks by Arendt (1968a, p. 34 n.) on the "undeserved honor of definitive biographies" of Hitler and Stalin:

> No matter how scrupulously Alan Bullock in his book on Hitler and Isaac Deutscher in his biography of Stalin followed the methodological technicalities prescribed by the genre, to see history in the light of these non-persons could only result in their falsifying promotion to respectability and in a more subtle distortion of the events. When we want to see both events and persons in right proportion we still have to go to the much less well documented and factually incomplete biographies of Hitler and Stalin by Konrad Heiden and Boris Souvarine respectively.

Arendt does not charge Bullock and Deutscher with holding the view of evil that is implicit in their treatments of Hitler and Stalin. Rather, she implies that they failed to consider this enormously important implication of their biographies, and are therefore culpable in the degree that people are always culpable for what they ought, but fail, to see. Things are worse when the intentions of veracity and responsibility cannot be granted a commentator, as when it is evident that a commentary distorts textual material in the interest of persuading readers to a position.

The emerging ideal of political commentary is more subtle than a narrowly exegetical understanding, and far more demanding of both commentator and critic. What it requires is that commentators be aware of, and take responsibility for, the worldly, political implications of their work. As for critics, the ideal makes their task far more difficult than the usual one of pointing out exegetical errors. It is not inherently hard to compare commentaries against original texts, to point out distortions, omissions, alternative readings, and so on. But it requires thought, and deliberate engagement in the enterprise of speaking the truth about the world, to fathom the implications of an interpretative statement.

Discussion has so far concentrated on the principles of veracity and moral responsibility towards others. The third criterion appropriate to commentary on the tradition of political theory is political judgment. To begin with, the just-discussed principles clearly lend themselves to specifically political applications, i.e., evaluations of the political force of commentaries in terms of the dual requirements of responsibility and insight. Surely it is legitimate, recalling Arendt, to question the political understanding conveyed by a commentary and, secondly, to judge commentaries for their practical import. In a field devoted to the study of politics, something odd is going on if it seems legitimate to contest arguments on exegetical—even moral—grounds, and not also on political ones.

In addition to legitimizing political debate, which is an obvious, if controversial, moral to the idea of political commentary, the idea further suggests a more subtle sort of political criticism: that is, criticism born of reflection on how commentary can be done well *as political discourse*. Self-consciousness and responsibility on the part of commentators for the political force of their work are part of the answer. A further question, which ideally requires an answer in the case of every sort of written work, is why the inquiry is carried out in the way it is. At best, it ought to be self-evident why a work takes the form it does, so self-evident that it is not only the work's arguments that persuade but also the light which its various aspects shed on one another. While this implies a pluralistic conception of good commentary, goodness being specific to the particulars of an interpretative project, there may be kinds of discourse to which commentary lends itself by virtue of its distinctive constitution. In other words, it is worth exploring whether commentary, as a genre, has distinctive advantages, even over theorizing in the grand, traditional style.

To be clear, the query concerns the advantages of commentary for political understanding. It is common to examine interpretative methods for their fitness to the purpose of understanding textual meaning. Here, the question rather concerns the fitness of commentary—and various varieties of commentary, specifically—for political analysis. And the relevant comparison is with grand theorizing (as well, of course, as with political science, but that is a separate topic).

Measured against grand theory, commentary is distinguished by three major characteristics: its use of interpretation as the medium of first-order argument; its narrowness, or limited scope; and its focus on the past, on the history of political thought. These differences provide negative clues: what is unlikely to be done well through political commentary. With some signal exceptions, notably the work of Machiavelli, the grand theories of the tradition cover a vast range of territory —epistemology, psychology, ontology. Commentaries are much more limited in scope, whether organized around a single theorist, an issue, a period, or whatever. The choice to theorize by talking about others' ideas makes narrowness a virtual inevitability, so much else in the way of first-order theorizing having to be given up as attention focuses on others' thought. Comprehensive, fundamental theorizing requires thoroughgoing reflection, equally, on the philosophical pieces (psychology and so forth) and on the whole they make up. Commentary, with its partial orientation towards understanding texts in the tradition, simply does not lend itself to synoptic analysis.

Concretely, the argument casts suspicion on two interpretative practices: *ex cathedra* criticism of the grand theories for erroneous views on human nature, the nature of knowledge, and so forth; and comprehensive histories of the tradition that categorize and pigeonhole everyone from Socrates to Marx and beyond. The first practice risks over-hasty evaluation, the too-ready assumption that where commentators have seen nonsense, "there is no sense which they have not seen" (Plamenatz, 1967, p. 22). It also risks the arrogant assumption that commentators have thought as much about the whole complex of parts that make up grand theories as did their authors. In a real sense, adequate criticism of a grand theory requires grand theorizing of equivalent depth and comprehensiveness. Somewhat similarly, the second practice effectively presumes that organizing and characterizing the tradition does not involve profound, philosophical issues. It was argued earlier that the notion of tradition is a useful but emphatically theoretical and abstract notion. It follows that one ought to be suspicious of categorizing histories which do not display an awareness of the theoretical nature of their categories and do not provide the necessary theoretical support for those categories.

By ordering particulars in the light of an incisive world view, comprehensive, detailed grand theory clarifies the world and opens it to action. Narrow, focused commentary can contribute to the same pursuits—clarity about the world and political action, although its means are different.

In illustration, listen to Leo Strauss on the utility of *selective* interpretation for understanding the present—the utility, specifically, of study of Hobbes for penetrating the illusions of liberalism. Strauss concludes a 1932 article on Carl Schmitt:

> The critique of liberalism that Schmitt has initiated can therefore be completed only when we succeed in gaining a horizon beyond liberalism. Within such a horizon Hobbes achieved the foundation of liberalism. A radical critique of liberalism is therefore possible only on the basis of an adequate understanding of Hobbes (Strauss, 1965, p. 351).

Hobbes can "teach us important truths which we have need of today and which we cannot find from any of our contemporaries" (Strauss, 1932-33, p. 610, my translation; cf. Strauss, 1963, p. 5). These statements imply one definition of commentary's purpose: exposure of the fundamental assumptions underlying commonly received ideas. The purpose requires selective interpretation, commentary on Hobbes and none other.

To generalize on Strauss's remarks: selective, historical commentary can function as an Archimedian lever with which to unhinge the illusions of the present. Certain theories in the history of political thought offer not simply an outside perspective on the present, but also the outside perspective that alone can clarify the present. From a critical standpoint, the rationale is a promise, kept or broken according to whether, in this instance, Strauss's actual interpretation of Hobbes truly does expose the lineaments of our thinking in the vision of the seventeenth-century master.

Secondly, there are advantages of specificity over comprehensiveness vis-à-vis political action. To begin with (whether Leo Strauss would agree is too complex a question to discuss here), clarifying exposures that illuminate a confused present are surely practical in some sense. The practical connection between commentary and action is more direct, though, when the tradition is used as a source of enlightenment on particular problems of pressing urgency. An instance is the issue which arose during the period of protest against the Vietnam War, the issue of one's obligation to die for the state, on which Hobbes's unfaltering elaboration of the logic of rights was especially insightful. Sometimes there truly isn't anyone better than a theorist in the tradition to instruct us about a political issue of immediate urgency, to teach us things "of which we have need today."

To sum up, I can find two practical guidelines that attach to the goal of self-evidence, i.e., to the idea that it ought to be clear in a commentary why interpretation is the chosen mode of analysis, and why commentary focused on these certain texts. Both guidelines take commentary's limited scope to be a virtue. Either the point is to focus on those specific theories that expose the foundations of ordinary political thinking, or it is to focus on those theories that uniquely instruct about contemporary political dilemmas. The list is not intended to be exhaustive. It is intended to suggest the need for reflection on the exact political utility of a particular interpretative project.

This last criterion—self-evidence—completes the trio of norms of political commentary. As has been said, these norms, veracity, responsibility, as well as self-evidence, are at least as demanding as those that attach to the exegetical purpose of understanding textual meaning. Indeed, one of the reasons, a further reason, the norms are so demanding is precisely that interpretation is the medium of political commentary. By definition, political commentary—commentary as political discourse—is a form of hidden writing. In the genre, arguments are made through the medium of talking about others' work. The bulk of discussion in typical commentary is exegetical in character, devoted to explicating textual meaning and defending a particular textual interpretation. Commentary's exegetical character inevitably, to greater or lesser extent, conceals its substantive argument. Because concealment is inherent to the enterprise, the contrasting virtues of truth-telling and responsibility need special emphasis. Hidden writing lends itself to manipulation of the reader, and it can foster blindness in the author to the implications of his or her enterprise. It is easy to use exegesis to make political arguments, while insisting throughout that the conclusions and rationale of the discussion are entirely exegetical in nature. Similarly, the fact that exegesis is the immediate and characteristic task of commentary can blind commentators to the larger—substantive—import of their work. These dangers of commission and omission require the countervailing ideal of commentary that integrates political and exegetical analysis in a self-evident, transparent whole.

The three propositions about political commentary outlined in this first section contradict common assumptions about interpretation. Commentary is usually viewed not as first-order discourse, but as second-order inquiry into the work of others; it is common, too, to insist upon the essential similarity of all interpretative inquiries and to play down the distinctive character of substantive varieties of interpretation, such as political commentary. Finally, understanding meaning is usually taken to be the goal and problem of interpretation, rather than the goals of truthfulness, responsibility and transparency in the argument of commentary itself. In the following section, the notion of political commentary will be contrasted with interpretative approaches to the study of political theory.

Historicism and Hermeneutics

There are two ways in which theories of interpretation are being applied to the study of political theory. The more controversial of the two is the historicist position of which the most pointed, polemical expression appears in the writings of Quentin Skinner and John Dunn. They would apply to political theory the restrictive doctrine that textual meaning is limited to the meaning that texts could have had at the time of their writing (Skinner, 1969, 1974; Dunn, 1968). What makes their position more than a recasting of traditional ideas about the importance of historical knowledge for textual commentary is its foundation in a theory of interpretation. At base, their work teaches the lesson that the important problems of political-theory commentary are interpretative in character, not political. It happens that they would solve those problems with an historicist argument, but their fundamental challenge lies in directing political theorists' attention to the theory and methodology of interpretation. The move links their challenge to the larger school of theorists who argue that political science itself ought to be understood as a human science of interpretation and not as a positivistic social science. Thus a second application of theory of interpretation consists in the use of the human-science idea as a model for political inquiry; hermeneutic theory is the example that will be used in the present discussion.

Although the basic threat to political commentary posed by historicism and hermeneutics is the same, a reorientation from politics to interpretation, it is important to note the differing origins and orientations of the two interpretative approaches. In general, as Gunnel has pointed out (1979, pp. 120-21), the history of philosophical accounts of the nature of inquiry in the human and social sciences is a history of continuing attempts to establish these studies as disciplines, the constant hope being that philosophy would provide them an identity and justification. The general scenario is accurate, in miniature, to postwar American political science. But because the historicist and human-science responses to empiricism developed along dissimilar lines, they issue in different alternatives to the customary political study of the tradition of political theory.

The historicist argument originated in dissatisfaction with customary histories of political theory of the sort exemplified by Sabine's classic text (Pocock, 1966, pp. 267-69). Drawing variously from Collingwood's philosophy of history, Austinian speech-act theory (Skinner, 1974, pp. 283-85), and the Kuhnian notion of "paradigms" (Pocock, 1973, pp. 13-19), Skinner, Dunn and Pocock mounted an effort to reform the history of political theory into a proper discipline by supplying the field with a methodology. Some controversy exists over the success of their methodological effort, whether in fact they have supplied a "procedure for practice" (Gunnell, 1979, pp. 102, 119-20).[4]

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[4] Gunnell's criticism should be contrasted with, among other statements, Pocock's claims for the methodology in *Politics, Language and Time* (1973, pp. 25-39). Pocock grants that "the universes of

Here, though, what is important is the contrast between their methodological purpose and the purpose of the philosophers of the human sciences in their quarrel with positivistic empiricism. Where the historicists conceive themselves as reformers of the study of the history of political theory, the human-science effort has been directed at theoretical statements concerning the nature of knowledge of human things. Indeed, deriving from Gadamer (1975), the major, contemporary hermeneutic philosopher, there is a pronounced anti-methodological strand within the human sciences (see Ricoeur, 1973; Gunnell, 1979, pp. 110-16). The contrast is not absolute. The historicist challenge rests, as will be seen, on a philosophy of interpretation and the human-science camp includes thinkers interested in the application of these philosophies to practice. However, adapting Gunnell's contrast between Sabinesque and historicist commentary (1979, p. 98) it is generally accurate to characterize the historicists as interested in *how* to study the history of political theory and the hermeneuticists et al as interested in "*what* should be studied" in the social sciences "and *why.*"

Historicism. Let us start with the historicist challenge to the customary practice of commentary on the tradition. I want to focus on the arguments of Skinner and Dunn, ignoring Pocock because his "paradigmatic" approach involves a distinctive series of concerns having to do with the ways in which societies conceive their past and with the various levels of abstraction characteristic of political thought (for example, 1962; 1973, pp. 3-41, 233-72). Skinner and Dunn imported an historicist theory of interpretation to political-theory commentary. Philosophical articles by both authors argue the approach, which, although the two differ in their points of emphasis, essentially consists in the following propositions.

First, historical and philosophical inquiry into the history of ideas must proceed in tandem. Historical analysis establishes the specific meaning of a proposition; only then can the proposition be analyzed for its truth or falsity, an analysis which ought, moreover, to be conducted in the terms of the time (Dunn, 1968, pp. 86-87, 90-96; cf. Skinner's attacks on the conception of "timeless truths" in the history of philosophy [for example, 1969, pp. 50-53]). Second, historical understanding requires evidence concerning what Skinner describes as two sorts of intentions, an author's intention "*to* do *x*" and the intention "*in* doing *x*" that can be attributed to a work in its context (1969, pp. 45-46). The "complex intention" formed of these two parts is the key to textual interpretation:

> The essential question which we therefore confront, in studying any given text, is what its author, in writing at the time he did write for the audience he intended to address, could in practice have been intending to communicate by the utterance of this given utterance (Skinner, 1969, pp. 48-49).[5]

By defining the meaning of texts in the tradition of political theory exclusively in historical, contextual terms, the argument yields the polemical point: "We must learn to do our own thinking for ourselves" (Skinner, 1969, p. 52).

Skinner's and Dunn's philosophical articles use examples from the history of philosophy and analytic stories to illustrate the theory.[6] Separate historical works apply the theory, in case-study fashion, to the interpretation of political-theory texts (for example, Skinner, 1972a, 1972b; Dunn, 1969). "The aims of my earliest methodological essays," Skinner writes in an autobiographical retrospective,

> were avowedly polemical. . . . My primary concern was to expose the weaknesses of two prevailing assumptions about the study of the classic texts in the history of political thought. . . . [M]y reason for attacking these methodologies was my sense that they had given rise to a number of exegetically plausible but historically incredible interpretations of the classic texts. *I took the case* of Hobbes's *Leviathan* and tried to show that this applied both to Warrender's (1957) and Hood's (1964) interpretations, as well as to Strauss's (1953) and Macpherson's (1962) accounts of Hobbes's place in seventeenth-century thought (1974, p. 279, emphasis added).

The particular historical interpretations (as well as the polemical injunction, "we must do our own

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discourses" in which a thinker may be involved are, in principle, "indefinitely numerous" (p. 28). Thus, it cannot be claimed for the methodology that it will issue in a single, definitive reading of a text. However, the methodology does direct inquiry to a specific, verifiable kind of data, "the 'language' or 'vocabulary' with and within which the author operated" (p. 25; see also pp. 30-38). I think it depends on one's expectations whether the approach, admittedly nondefinitive but nonetheless directive, qualifies as a methodology.

[5] In later articles, Skinner downgrades the importance of "biographical" intentions (i.e., the intention *to* do *x*), giving preference exclusively to the "contextual" intention *in* doing *x* (e.g., 1974, pp. 284-85). Dunn, on the other hand, emphasizes biographical intentions (1968, pp. 98-99).

[6] Compare Skinner, 1969, which employs historical examples, with his 1971 article in which the argument is entirely analytic.

thinking") have substantive, political import. Dunn, for example, concludes *The Political Thought of John Locke* with the assertion that Locke cannot help us resolve the confusions engendered by the decline of liberalism: "We have, it seems, come to accept in the broadest of terms the politics of Locke but, while doing so, we have firmly discarded the reasons which alone made them seem acceptable even to Locke. It is hard to believe that this combination can be quite what we need today" (1969, p. 267). Such political implications, it must be emphasized, derive from the application of a theory and methodology of interpretation to political material. They are not internally developed on the basis of reflection on this particular body of literature.

Not surprisingly, the historicist approach therefore tends also to dissolve the political interest of the classic texts. To see the point in the concrete, compare Dunn's evaluation of Locke's present-day significance with Macpherson's concluding argument in *The Political Theory of Possessive Individualism*. Macpherson, to recall, argues that liberal theory could provide an adequate account of political obligation so long as the assumptions of possessive individualism held in reality. But with the emergence of a class-conscious, politically articulate working class in the nineteenth century, those assumptions ceased to prevail. "It follows that we cannot now expect a valid theory of obligation to a liberal-democratic state in a possessive market society" (1964, p. 275; pp. 271-77, passim). Dunn's evaluation rests on the parallel argument that religious belief is a necessary condition for the cogency of Locke's social and political theory; that condition having disappeared, a rational, Lockean account of political obligation can no longer be given (Dunn, 1969, pp. 256-67). These logically parallel arguments capture the difference between political and historicist commentary. Macpherson's commentary holds political interest for a contemporary theorist; Dunn's commentary makes Locke archaic and irrelevant. Of course, Macpherson's sort of argument must be grounded in textual and historical empirical evidence, a point on which Dunn takes him to task (1969, pp. 255, 262-66). But, granting Locke to be a political theorist, mustn't Dunn's sort of argument, similarly, be taken to task for its political irrelevance?

Hermeneutics. Employed in a different theoretical manner, a preoccupation with interpretative understanding can have the effect of dictating modes of political theorizing. The application of theory of interpretation to the social sciences generally can be explored using materials from a variety of interpretative schools. Major candidates include hermeneutics, narrowly defined as the theory of textual interpretation; "critical theory" or "sociology," i.e., the work associated with the Frankfurt School; phenomenology; and ordinary-language philosophy, as well as speech-act theory, the schools deriving from Wittgenstein and Austin (see Bernstein, 1978; Connerton, 1976; Dallmayr and McCarthy, 1977). For students of politics, more particularly, writings by Taylor (1971), Habermas (for example, 1970a, 1970b), and Ricoeur (1971) hold special interest. By a loose definition of hermeneutics as a general philosophy of interpretation, these authors are all hermeneuticists, and are all linked in one way or another to Gadamer.

To begin examination of the case for hermeneutics, consider the obvious utility of hermeneutic theory as a foil for the historicist philosophy of interpretation. Set against restrictive interpretative arguments such as the historicist doctrine that limits textual meaning to contextual meaning, hermeneutics has the initial attraction of a permissive theory of understanding, capable of use in defending trans-historical interpretation of the tradition of political theory. Hermeneuticists conceive themselves engaged in conversation with the texts they study: interpretation consists in constant revision of the interpreter's prejudices in light of what is learned from the texts, the final aim being an understanding both faithful to the texts and responsive to the interpreter's concerns. The hermeneutic ideal is analogous to the dialectic of conversation: a mutual understanding between two partners, founded in sympathy and productive of a synthetic truth that comprehends and goes beyond their two, limited perspectives (Gadamer, 1975, pp. 325-33):[7]

> Every age has to understand a transmitted text in its own way, for the text is part of the whole of the tradition in which the age takes an objective interest and in which it seeks to understand itself. The real meaning of a text, as it speaks to the interpreter, does not depend on the contingencies of the author and whom he originally wrote for. It certainly is not identical with them, for it is always partly determined also by the historical situation of the interpreter and hence by the totality of the objective course of history. . . . Not occasionally only, but always, the meaning of a text goes beyond its author. That is why understanding is not merely a reproductive, but always a productive attitude as well. . . . It is enough to say that we understand in a different way, if we understand at all (Gadamer, 1975, pp. 263-64).

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[7] However, Gadamer also points to differences between interpretation and conversation (1975, p. 349; see, also, 1977, pp. 21-26, and Ricoeur, 1971).

However, while hermeneutics is specifically a philosophy of textual interpretation, originally scriptural interpretation, it has largely been applied in the social sciences as a guide for first-order inquiry. Despite Gadamer's anti-methodological position (for example, 1977, pp. 26-27, 39-40), others have attempted programmatic statements outlining a hermeneutical social and political science. Ricoeur (1971) builds the case with discussion of the obviously central questions: "Are texts a good paradigm for the objects of the human, social sciences?" and "Is the methodology of text-interpretation useful as a paradigm for interpretation in the human sciences?" (pp. 529-30). Taylor (1971) addresses the same general issues (as on pp. 10-17), but goes on to specific discussion of the idea of a hermeneutical political science. Taking on empirical political science, he identifies features of reality which "fall through its net," i.e., intersubjective and common meanings. Taylor focuses, as have Macpherson and Dunn, on the contemporary issue of legitimacy (pp. 35-45): "My principal claim is that we can only come to grips with this phenomenon of breakdown by trying to understand more clearly and profoundly the common and intersubjective meanings of the society in which we have been living" (p. 44). Thus, in contrast to the historicists, these hermeneuticists raise the questions: "Why interpret the history of political theory? Why not go directly to political reality, and use theory of interpretation as a guide for interpreting that reality?"[8] Meta-level interpretative analysis here stands not opposed to immediate political theorizing, but definitive of it.

There are two replies to be made to the hermeneuticist queries. First, as in the historicist case, the idea of a hermeneutical political science focuses debate on interpretative issues. Second, when applied to political analysis, hermeneutic theory issues in arguments lacking foundation in political reflection. Surely, political thinking is best developed precisely by reflecting on politics, including the taking of instruction from the masters, rather than by reflecting on interpretation.

Consider Habermas's critique of Arendt. Habermas (1977a) recasts Arendt's idealization of the *polis* united by its citizens' "constant interchange of talk" (Arendt, 1968a, p. 24) into his own language of "communicative action" (for example, pp. 7-9). He then goes on to criticize her vision as narrow (pp. 21-22), a criticism which has itself been questioned in a recent article (Knauer, 1980, pp. 730-31). But at a very down-to-earth level, isn't a vision like Arendt's, a vision built up on reflection about politics, quite simply preferable as political theory to Habermas's dense treatments of political ideas in linguistic language? Kress, citing Habermas, makes the point in a most biting fashion that interpretative theory is hardly a direct route to political insight. Why is it "felt by many serious scholars," he asks, "that, e.g., a science of hermeneutics, psychoanalysis, and distorted communication is needed in order to write significantly about nuclear disarmament, the imperial presidency, or Washington secretaries without typing skills?" (1979, pp. 533-34).

In its way, the hermeneutic approach diminishes political theory fully as much as does the historicist argument. Both direct attention to the nature, conditions and difficulties of interpretative understanding, such issues at far remove from the political problems that preoccupied the theorists of the tradition. The further problem in the hermeneutic case, because the theory has been applied as a model for inquiry, is its issuance in political arguments whose foundation is nonpolitical. Surely the tradition of political theory would be a better guide and source. But the tradition can only be that, to summarize the present argument, if it is approached from a political and not an historicist perspective.

There is one further, shared flaw in these interpretative theorists' approaches to political theory: a lack of self-consciousness regarding the import of their work. Skinner and Dunn emphasize the contextual meaning of the theories of the tradition, but they avoid submitting their own work to the same analysis (see Skinner, 1974, p. 281; Gadamer, 1975, pp. 266-67, elaborates this criticism of historicism). Habermas and Gadamer have quarrelled in print over the issue of whether the characteristic hermeneutical concern with appropriating and continuing traditions spells an inherent conservatism in the philosophy. Habermas maintains the need "for a reference system that goes beyond the framework of tradition"; "only then," he continues, "can tradition also be criticized" (1977b, p. 358). Gadamer replies that there is no outside position from which to criticize past and present; that Habermas's emphasis on critique represents a new, anarchistic dogmatism; and that hermeneutics supplies the proper attitude of choice vis-à-vis tradition and authority (Gadamer, 1977, pp. 26-42). However, both take as unexceptional their shared assumption, exemplified in the use of conversation and translation as central analogies, of a consensual ideal of human interaction and community. Both see misunderstanding or distorted understanding as the central problem of human life (for example, Habermas, 1970a; Gadamer, 1977, p. 25). The viewpoint obviously derives from the interpretative framework within which they work. But a political theorist, apprenticing with the classics, would know to measure any such assumptive ideal against alternative angles of vision.

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[8] Taylor, it has to be noted, would presumably quarrel with this "either/or" formulation, either interpret the classics or interpret the world. His *Hegel* book obviously falls within the genre of commentary on the tradition. Furthermore, in the preface to its condensation as *Hegel and Modern Society*, he emphasizes the relevance of Hegel's political philosophy to our time (1979, pp. xi-xii). The argument effectively identifies the work as a "political commentary."

Historicist interpretation, oriented towards recovery of original meaning, disregards the gap between its task and its import as an argument in its own right. It does so by confusing a coherent methodological and philosophical argument with a solution to the inherent interpretative tensions between self and other. Historicism provides a theory of interpretation with which to carry out the task of understanding meaning; this theory, in turn, conveys an understanding of the world of meaningful human action. The two dimensions do form a coherent whole that rationalizes both the methods and significance of inquiry. However, historicist theory and practice are silent on the middle range of substantive issues, between methodology and philosophy. The approach, when applied to the study of texts in political theory, does not orient reflection toward the nature of politics. As a result, the substantive rationale of particular inquiries is neglected: the reasons why it is important to study certain texts, with certain questions in view, and so on. It seems to be implicitly assumed that this middle range of theorizing is covered by the philosophical dimension of historicism. Yet, the point is that applications of the approach have "middle-range," substantive significance, a significance which is not framed in middle-range, substantive argument. Here, the tension between the task of interpretation and the autonomous significance of interpretations would reappear, only it is not addressed because historicism fosters the illusion that substantive import is an unproblematic derivation from historicist methodology.

Hermeneutic political theory also misses a distinctive kind of political question, this time a theoretical question: What constitutes the best analytic structure with which to attack diagnosis of the political world? Again, the problem arises through confusion of a coherent interpretative philosophy for a self-evident framework of social and political analysis. Hermeneutics is silent on a middle range of substantive, theoretical questions, those having to do with the choice of concepts around which to orient theoretical analysis—freedom, justice, right, utility, consent, peace, to name some obvious candidates.

The idea of political commentary provides an alternative, and a solution, to the flawed historicist and hermeneutic approaches to political theory. Its view of interpretative exegesis as a subsidiary part within first-order political argument does not dissolve the tension between these two dimensions, but it does provide a reasonable perspective on the tension, a perspective that leads beyond epistemological preoccupations. Its insistence on the instructiveness of the classics will not issue in a ready-made blueprint for political theorizing. To the contrary, the idea, like the tradition, teaches the lesson that political thinking is an art. There is no general solution to the problems of combining textual exegesis, phenomenal analysis and political argument. These problems are only to be solved, again and again, by the art of particular commentators who manage to wed exegesis and reflection in commentaries that are simultaneously as self-evident, responsible and truthful as is humanly possible.

Implicit in this discussion of political commentary is one final criticism of interpretative approaches to political theory. Historicism and hermeneutics are abstract theories having broad application to many fields of inquiry. As such, they effectively limit the possibilities of political theory. The aspiration to theoretical insight and originality on the order of the classic texts is better served by study of the classics themselves. For these reasons, political theorists have been wrong to look to interpretative theory for guidance on the definition of their enterprise. The only fully satisfactory commentary would be one which synthesized interpretative rationale, application and findings in a political vision not unlike the visions of the great theories.

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The Decline of Political Partisanship in the United States: Negativity or Neutrality?

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This article examines attitudes towards the two major political parties in the United States from 1952 to 1980, using national election study data from open-ended likes/dislikes questions. The major trend which is found is a shift toward neutral evaluations of the parties. A reinterpretation of party decline in the electorate is offered, in which the much-discussed alienation from parties is largely rejected as an explanation. Rather, it is argued that the link between parties and candidates has been substantially weakened over the years and hence that political parties have become increasingly meaningless to the electorate.

One of the most widely discussed topics in political science over the last decade has been the decline of partisanship in the American electorate. Increasing numbers of voters are declaring themselves political independents and splitting their tickets. Many read in these trends "the end of parties," at least as we know them. That we now live in an "anti-party age" is beyond dispute, according to many authorities. As Jeff Fishel (1978) has noted, both proponents of parties as vote-maximizing and consensus-building institutions, such as Jeane Kirkpatrick and Austin Ranney, and those who favor the responsible party government model, such as Gerald Pomper and David Broder, agree that American parties have come to a "shambles."

Yet evidence indicates that in certain respects there has been no decline of parties at all. As organizations, parties have suffered from the disappearance of the big city machines, but overall there are indications of continued organizational vitality. Data from the national election studies conducted by the Survey Research Center/Center for Political Studies (SRC/CPS) at the University of Michigan show that an increasing percentage of citizens have been reporting that they have been exposed to local party organizational contacts (Wolfe, 1979). According to Jewell and Olson (1978), state political party organizations have been revitalized, with the result being a more active role for the state parties. And on the national level, Cotter and Bibby (1980) have concluded from their extensive study of the history of national party organizations that both parties have undergone processes of institutionalization and nationalization in recent years. As for parties in government, party affiliation remains an excellent predictor of voting behavior in Congress. In addition, recent congressional reforms have served to increase the potential for party leadership and have restored the party caucus (Dodd and Oppenheimer, 1977). Thus it is evident that the decline of parties in the electorate is a much clearer trend than the decline of parties as organizations or in government. Therefore, to make generalizations about parties being in a shambles may lead us to overlook the importance of aspects of party strength which have not declined.

What is most curious about all of these countertrends, however, is how they can be occurring at the same time when voters are not only displaying a greater independence from party, but also becoming more and more alienated from them according to Nie, Verba and Petrocik (1976, pp. 57-58). It is the central point of this article that the increase in alienation towards the parties has been minimal. The major change which has taken place in the public's evaluations of the parties has been towards a neutral attitude rather than a negative one. This is not to say that there has not been a decline of parties *in the electorate,* but instead that the nature of the decline has been different from what many have assumed. Such a distinction is crucial for any understanding of the future of American political parties. If voters are alienated from political parties, then the parties' chances for recovery in the near future are doubtful, but if people are only neutral towards them, then the door remains open for party renewal.

Indeed, a good deal of speculation about the future of the political party system rests on the assumption that voters have rejected parties. For example, Kristi Andersen (1976, p. 95) writes that

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a major difference between the 1920s and 1970s is that in the 1920s those not affiliated with a party were largely apathetic while in the 1970s "there appears to be a more principled rejection of parties." Such a repudiation, she argues, will make the capture of Independents by one of the parties "exceedingly difficult." It is thus crucial to first assess whether such an alienation process has in fact been responsible for party decline in the electorate.

An Examination of the Alienation Hypothesis

The alienation hypothesis concerning party decline in the electorate contains two basic components. First, it has been assserted that voters increasingly see fewer important differences between the Democrats and the Republicans. Or, as George Wallace said in 1968, "There's not a dime's worth of difference between the two major parties." Second, it has been presumed that as distrust of the government—i.e., political cynicism—has increased concurrently with the rise in voter independence, that the two trends are related. One of the clearest arguments for the joint impact of these causal factors has been offered by Nie, Verba and Petrocik (1976, p. 283). They attribute the increased expressions of disenchantment with the government to the troubles of the late 1960s and proceed to describe the following sequence of events:

> The issues of the 1960s . . . do not clearly coincide with party lines; thus the parties offer no meaningful alternatives that might tie citizens more closely to them. Thus the political parties reap the results of the disaffection. Citizens come to look at the parties in more negative terms; they also begin to abandon the parties in greater numbers.

However, the data which are available from the SRC/CPS election studies do not provide much support for such an interpretation. To begin with, as Table 1A shows, the proportion of respondents who thought that there were "important differences in what the Republican and Democratic parties stand for"[1] has remained quite stable over the years. Between 1952 and 1976 the proportion seeing important differences fluctuates minimally in the range from 46 to 52 percent. The data for 1980 show a significant change from 1976, but it is in the opposite direction from what would be predicted by the alienation hypothesis, with 58 percent now perceiving important differences between the parties.

Nevertheless, the fact that the electorate continues to see important differences in what the parties stand for does not necessarily mean that citizens continue to see the differences as meaning anything in terms of government performance. Since 1960 the election studies have asked respondents what they considered to be the most important problem facing the country. Those who mention a problem are subsequently asked which of the two parties would be the most likely to do what they want on this problem.[2] As can be seen in Table 1B, the percentage who feel that one of the parties will do better on the problem which most concerns them has declined since 1964. In that year, 65.7 percent thought that either the Democrats or Republicans would be the most likely to do what they wanted, even though only 50.8 percent said that there were important differences in what the parties stand for. In contrast, in 1980 only 50.3 percent thought one party would be better on the problem they considered most important—which for the first time is significantly lower than the proportion seeing important differences between the parties.

One possible interpretation of this finding is that it represents a growing disenchantment with political parties and the government in general. Black and Rabinowitz (1980, p. 241), for instance, note that it bears a striking similarity to trends in trust in government. They write that "if neither party can provide desirable alternative solutions to the problems an individual feels are most important for the government to do something about, it is reasonable for the individual to view the parties unfavorably and lose faith in the government." According to such an interpretation, respondents may still see major differences between the parties, but many are so dissatisfied with the opposing alternatives that they have become alienated.

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[1] The wording of this question has changed slightly over the years. Listed below are the different versions which have been asked:

1952: "Do you think there are any important differences between what the Democratic and Republican parties stand for, or do you think they are about the same?"

1960, 1964, 1972-1980: "Do you think there are any important differences in what the Republicans and Democrats stand for?"

1968: "Do you think there are any important differences between the Republican and Democratic parties?"

[2] This question has also undergone slight changes in wording over the years. In 1960, 1964 and 1968 respondents were asked which party they thought would be the most likely to do what they wanted on whichever problem they mentioned. In 1972 this was changed to asking which party would be the most likely to get the government to be helpful on the problem, and in 1976-1980 the wording was again changed to which party would be the most likely to do a better job in dealing with the problem.

However, those respondents who believe that there wouldn't be any difference between the parties in handling the problem they feel is most important are no more cynical than those who believe that there would be a difference. On a cynicism scale ranging from +100 to —100, the former group is never more than two points more cynical than the latter. In fact, those respondents who feel that there would be no difference between the parties are actually somewhat less cynical in both 1976 and 1980 compared to their counterparts in these years.

Such a null finding brings into question whether there is any relationship between the respective declines in political partisanship and trust in government, as is often presumed. One simple way to establish whether there is such a relationship is to correlate the cynicism scale with strength of party identification. Only in 1968 is there a relationship worth noting. The Pearson correlation between the two variables varies from a high of .11 in 1968 to a low of —.02 in 1972, averaging about .04. Therefore, it may be plausible to infer that the initial declines in both party identification and trust in government between 1965 and 1968 were slightly intertwined, but there has been no consistent relationship between the two variables.

Yet cross-sectional data can hide systematic changes over time. As Nie et al. emphasize, panel data are necessary to establish or reject a causal link. Fortunately one panel is now available in which there are substantial declines in both party identification and trust in government—the 1965-1973 Jennings-Niemi socialization panel.[3] This data set is particularly interesting because it contains a large sample of new entrants into the electorate who have contributed so much to the growth of political independents (Abramson, 1976).

If it is true that the decline in party identification is due to the growth of cynicism, then it would be expected that those in the panel who

Table 1. The Electorate's Perception of Differences Between the Two Major Parties, 1952-1980 (Percent)

A. Percent Seeing Important Differences in What the Democrats and the Republicans Stand For

	Important Differences	No Differences, Don't Know[a]
1952	49.9	50.1
1960	50.3	49.7
1964	50.8	49.2
1968	52.0	48.0
1972	46.1	53.9
1976	47.2	52.8
1980	58.0	42.0

B. Percent Seeing One of the Parties as Doing the Best Job on the Problem They Consider Most Important

	Party Mentioned[b]	About the Same	Don't Know
1960	62.0	25.0	13.0
1964	65.7	25.3	9.0
1968	51.6	38.7[c]	9.8
1972	49.0	42.4	8.6
1976	46.3	46.0	7.7
1980	50.3	42.7	7.1

Source: National election studies conducted by the University of Michigan's Survey Research Center/Center for Political Studies.

[a] In 1964 and 1968 "don't know" was not on the questionnaire; thus the percentage coded as "no difference" was artificially inflated. To make all results comparable, I have combined responses of "no difference" and "don't know."

[b] Respondents mentioned either the Democratic or Republican party.

[c] Included are those who saw no difference between the major parties but believed that Wallace would do what they wanted.

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[3] I wish to thank M. Kent Jennings for making these data available to me, as well as for offering advice concerning their use.

became more distrustful between 1965 and 1973 would also be the respondents who showed the greatest decline in partisanship. But, as Table 2 demonstrates, this was not the case. For example, data on the younger generation (first interviewed as high school seniors in 1965) shows that there was a substantial decline in strength of party identification in every cell of the table with very little variation depending on whether the cell represents those whose cynicism score increased, stayed the same, or decreased. Even the youth who were *least* cynical in both years showed a sizable decline of 24.4 points on the strength of party identification measure. In comparison, those who shifted from trusting to cynical during the eight-year interval showed only an 18.5-point drop. For their parents, the decline in strength of party identification is much less, but there is a similar lack of any consistent pattern of changes by cynicism scores. Indeed, some of the cells show changes which are the reverse of what would be expected.

Thus there is no evidence in this panel to suggest that the rise in cynicism towards the government has been responsible for the decline of partisanship. It must therefore be concluded that the growth of cynicism and political independence are roughly parallel trends but which have little relationship to each other in both a static and dynamic sense.

In summary, what has been found thus far suggests that a reexamination of citizens' attitudes towards the parties is in order. The alienation hypothesis depends largely on two assumptions for which little support has been found here: that voters no longer see important differences in what the parties stand for and that the decline in strength of party identification has been an outgrowth of the decay of public trust in the government.

It is true that there has been a decline in the percentage of the electorate which believes that one party would do a better job in handling whatever they perceive to be the most important problem facing the government. However, the fact that such feelings are not related to political distrust indicates that this trend may not necessarily reflect negative attitudes towards the parties. An equally plausible alternative explanation is that citizens simply see the parties as less relevant than in the past and hence their feelings towards them are more neutral than negative. With the growth of the mass media and candidate-centered campaigns, the importance of parties in the presidential selection process and government in general has been weakened. The ideological differences between the parties may re-

Table 2. Change in Strength of Party Identification by Change in Cynicism Toward the Government, 1965-1973

	1973 Cynicism		
1965 Cynicism	**Trusting**	**In-Between**	**Cynical**
Youth			
Trusting	−24.4	−36.3	−18.3
N	(206)	(307)	(281)
In-Between	−16.3	−29.4	−22.1
N	(49)	(108)	(160)
Cynical	*	−24.8	−30.2
N	(8)	(28)	(63)
Parents			
Trusting	−9.9	+4.3	−13.9
N	(153)	(206)	(137)
In-Between	+2.1	−12.2	0.0
N	(51)	(132)	(157)
Cynical	*	−6.1	−17.7
N	(16)	(84)	(154)

Source: Panel Study of Generations and Politics conducted by M. Kent Jennings and Richard G. Niemi.

Note: Table entries represent change in strength of party identification from 1965 to 1973. Strength of party identification is calculated by scoring the percentage of Strong Democrats and Strong Republicans as +2, Weak Democrats and Weak Republicans as +1, Independent Democrats and Republicans as 0, and Pure Independents and Apoliticals as −1.

*Insufficient data.

main, but on the crucial short-run policy issues of the day it is the candidates which now matter most. Because of these changes, there is reason to expect that the electorate should be less positive about the two parties than in the past but that the shift in attitudes will be toward neutrality rather than negativity. Just such a possibility will be examined next.

An Evaluative Perspective on Party Decline

One major weakness in the literature on party decline is that the research has been nearly wholly concerned with the affective (party identification) and behavioral (voting by party line) aspects of the electorate's relationship to political parties. This is not to demean the importance of the rise of Independents and split-ticket voting. Clearly such trends may forebode significant systemic consequences, most notably the rise in political volatility and instability in the U.S. The problem is that without proper evidence such trends have been grafted onto the description of the evaluative dimension of parties as well. That more people choose to call themselves Independents may not mean that they are "rejecting" parties, or, as James Sundquist puts it (1973, p. 343), "calling down a plague on both their houses." Being an Independent may mean to most voters just what it says—being independent of parties.[4] Choosing to split one's ticket also does not necessarily imply a long-term rejection of parties. As Miller and Levitan have argued (1976), much of the increase in ticket splitting simply reflects the fact that specific presidential candidates have been nominated whom many party members could not support. Thus it may well be a better indicator of the ability of each party to select its nominee than of voters' evaluations of the parties.

What is sorely lacking is an analysis of just how positive, neutral or negative Americans are towards the parties and what they like or dislike about them. Such an evaluative approach is presented here. In every presidential election year since 1952, the SRC/CPS election studies have asked respondents in the pre-election wave what they liked and disliked about the two political parties. Up to five responses have been coded in each year for each of the four questions.[5] By simply subtracting the number of negative comments from the number of positive ones, we can classify respondents as either positive, neutral or negative toward each of the parties, depending on whether the number of likes exceed, equal or are fewer than the number of dislikes.

After collapsing the data into negative, neutral and positive ratings on each of the two parties, I created a sixfold classification representing respondents' ratings of both parties combined. The six categories are as follows: (1) *negative-negative:* those who have negative attitudes towards both parties; (2) *negative-neutral:* those who report negative ratings of one party, a neutral evaluation of the other; (3) *neutral-neutral:* those who are neutral with respect to both parties; (4) *positive-negative:* those who report a positive evaluation of one party, a negative evaluation of the other; (5) *positive-neutral:* those who are positive towards one party and neutral towards the other; and (6) *positive-positive:* those who rate both parties positively. It should be noted that in all of these categories it is irrelevant which party the respondent feels warm, neutral or cold to. For example, some of the *positive-negative* respondents rate the Democrats positively and the Republicans negatively and some vice versa, but what is important here is that these respondents see the parties in a polarized warm-cool fashion, not which party they are positive towards.

The percentage of respondents falling into each of the six categories of attitudes towards the two major parties on the basis of these open-ended likes/dislikes questions from 1952 to 1980 is shown in Table 3. If it is true that citizens are alienated from the parties and have come to perceive them in far more negative terms than in the past, then one would expect to find a large increase in the proportion of *negative-negatives* and *negative-neutrals* in the post-1964 period. Certainly this is the case in 1968, the first measurement point available after party identification began to decline. From 1964 to 1968 there is a 5.6 percent increase in those negative towards both parties and a 2.6 percent rise in the *negative-neutral* category. Given that partisanship continued to erode after 1968, it might be hypothesized that the percentage of the electorate with negative attitudes towards the parties would also continue to rise. However, Table 3 clearly shows just the reverse. Since 1968 both the *negative-negatives* and *negative-neutrals* have declined. Thus, the 1968 election stands out as an aberrant year with respect to negative attitudes about the parties rather than the beginning of a trend.

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[4] A related argument is that of Herbert Weisberg (1980), who presents evidence from feeling thermometers which indicates that the party identification measure taps three dimensions—one measuring attitudes toward the Republican party, another measuring attitudes toward the Democratic party, and a third tapping attitudes toward political independence.

[5] In 1972 CPS originally coded only the first three responses. The fourth and fifth responses were later coded by Arthur H. Miller, whom I wish to thank for making these data available to me.

What does change dramatically after 1968 involves the large increase of the *neutral-neutrals* and the decline of polarized partisans, i.e., the *positive-negatives*. In 1968 the proportion of polarized partisans was over twice that of the *neutral-neutrals;* in both 1972 and 1976 the proportion of *neutral-neutrals* is roughly equal, and in 1980 over 9 percent higher. But what is most fascinating about Table 3 is that the increase of those having neutral attitudes towards both parties is a trend which is evident throughout the entire 28-year period—in what Converse (1976) has termed the "steady state" period as well as in the period of weakening ties to the parties. From 1952 to 1964, while strength of party identification showed little change, the proportion of *neutral-neutrals* increased with each election, from 13.0 percent in 1952 to 20.2 percent in 1964. Only in 1968 is the linearity of the trend broken due to the largely short-term increase in negative attitudes towards the parties in that year.

It is also evident from Table 3 that the decline of polarized partisans began well before strength of party identification started to drop off. This is especially apparent between 1952 and 1956 when the proportion of *positive-negatives* fell by over 10 percent. One would intuitively have to hypothesize that the issues of the New Deal, on which the party system is generally considered to have been aligned upon, were sharply declining in salience from 1952 to 1956. Through 1960 many of these polarized partisans apparently moved to the less polarized categories of *positive-neutral* and *positive-positive,* both of which reach their high points during the years of the Eisenhower presidency. But after 1960 these groups decline in numbers as well as the *positive-negatives.* Overall, from 1952 to 1980 the percentage of *positive-negatives* falls from 50.1 to 27.3 percent. Except for the slight increase during the Eisenhower years, the percentage of respondents positive towards both parties remains fairly stable at about 5 percent.

Given the evidence from Table 3, the reader may wonder how Nie, Verba and Petrocik concluded that citizens had come to look upon parties in more negative terms—especially because they also analyzed responses to the party likes/dislikes questions.[6] The answer simply is that Nie et al. combined the categories of *negative-negative, negative-neutral* and *neutral-neutral* into a single group which they label variously throughout the book as "negative evaluations of both parties," "alienated from the parties," or "nonsupporters of the parties." Only the third phrase is an accurate description of what they are measuring. To infer that neutral evaluations of the parties represents alienation is to make an extremely tenuous assumption.

The question which this finding poses is of course what have been the forces causing members of the electorate to be more neutral towards the two parties. Data from the likes/dislikes probes are particularly useful for investigating this question because respondents are free to express exactly what their likes and dislikes concerning the parties actually are. It is these substantive comments which we will examine next in order to find

Table 3. Trends in the Public's Evaluations of the Two Major U.S. Parties, 1952-1980 (Percent)

	Negative-Negative	Negative-Neutral	Neutral-Neutral	Positive-Negative	Positive-Neutral	Positive-Positive	N
1952	3.6	9.7	13.0	50.1	18.1	5.5	1799
1956	2.9	9.0	15.9	40.0	23.3	8.9	1762
1960	1.9	7.5	16.8	41.4	24.2	8.3	1164
1964	4.4	11.2	20.2	38.4	20.6	5.0	1571
1968	10.0	13.8	17.3	37.5	17.4	4.1	1557
1972	7.9	12.6	29.9	30.3	14.7	4.7	1372
1976	7.5	11.8	31.3	31.1	13.7	4.5	2248
1980	5.0	8.6	36.5	27.3	17.7	4.8	1614

Source: National election studies conducted by the University of Michigan's Survey Research Center/Center for Political Studies.

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[6] There are several technical differences which should be noted between the likes/dislikes data which I have used and those Nie et al. use. First, Nie et al. use only the first three responses in all years, while I employ all five. Second, they fail to filter out the 100 respondents in 1952 and the 17 in 1960 who were not interviewed in the pre-election wave. As these respondents were not asked the likes/dislikes questions, they should be excluded from the analysis. Finally, Nie et al. include responses from the black supplements in 1964 and 1968. Subsequent analysis at CPS has shown that these supplements actually introduce a greater degree of bias into the sample. Hence I have not used them.

a fuller explanation for the trends which have been discovered thus far.

A Dimensional Analysis of the Likes/Dislikes Data Comparing Party and Candidate Responses

As suggested earlier, one possible explanation for why parties have declined in importance is that for various reasons candidates no longer need the parties in order to win elections. As Lester Seligman has written (1978, p. 300), "Deprived of presidential support, political parties are losing their meaning to the voters. Increasingly, Presidents are no longer making political parties and parties are no longer making Presidents." Also, previous voting research (Kirkpatrick et al., 1976; Miller and Miller, 1976) has shown that the effect of candidate images on the vote has increased over the years, while the effect of party images has declined.

The question which therefore needs to be examined concerns the type of open-ended responses on which candidates have assumed greater saliency, i.e., importance, than parties. Just such a pattern might be expected on specific issues. It will be recalled that when respondents are asked which major party would do best in solving whatever they consider to be the most important problem of the day, an increasing proportion have been responding that there would be no difference. This trend cannot be explained by an increase in the perception that there are no longer important differences between the parties, for there has not been any such increase. Nor can it be attributed to the decline in trust in government. Thus it might be hypothesized that more citizens are now exclusively conceptualizing issues in terms of candidates only and less in terms of party alone. In other words, the stands which candidates take on the issues may no longer be linked to voters' perceptions of the parties. The parties may still stand for certain broad principles and groups, but when it comes to specific policies, candidates now stand above parties rather than with them.

To test this hypothesis, we can compare the frequency of responses to the candidate likes/dislikes questions to the frequency of likes and dislikes expressed about the parties on four substantive dimensions or response categories: domestic issues, foreign issues, benefits provided to groups, and general political philosophy.[7] If such a hypothesis is correct, then one would expect an increase over time in the proportion of the population commenting about issues with respect to the candidates but not with respect to parties. Such a pattern would indicate that issues are irrelevant to the citizen in evaluating parties but relevant with regard to the candidates.

Table 4 provides evidence which shows that such a pattern has become much more prevalent over the years, especially on domestic and foreign policy. For example, domestic issues were overwhelmingly associated with the parties in 1952, with only 2.7 percent making such comments with reference only to candidates compared to 53.5 percent for only parties. But by 1964 the balance had shifted dramatically to the point where more respondents were mentioning domestic issues only in terms of candidates than parties only, and in 1980 the ratio is over two to one in favor of the candidate-only response pattern. References to parties and candidates on foreign policy were roughly balanced in 1952, but the same trend is nevertheless apparent. By 1964 the percentage in the candidate-only category was twice that of the party-only group, and in 1980 41.6 percent of the sample mentioned foreign policy in reference to a candidate but not with respect to either party, while only 3.5 percent displayed the reverse pattern. Finally, while there is a slight decrease in the party-only response pattern for the dimensions of group benefits and general philosophy of government, these attitudes still remain predominantly the domain of the parties, and thus probably account for the lack of any drop in the percentage of the population perceiving major differences between the parties.

Thus it seems that leadership is one crucial variable in determining why the electorate has become more neutral towards the political parties. As candidates have come to assume a much larger share of the spotlight on domestic and foreign policy the parties have seen much of their base of support erode. Apparently the likes and dislikes which citizens perceive for leaders on the issues are not being translated into similar attitudes towards the parties as well.

In an attempt to advance the argument that leadership is one of the key reasons behind the rise in neutrality, we can also examine responses concerning leaders on the *party* likes/dislikes questions. If the explanation for party decline is simply that candidates have become more salient, then one might expect that more people would be referring to leaders when asked about their likes and dislikes of the parties, even though other types of responses on the party likes/dislikes questions have decreased. But if the decline is also due to the fact that the link between parties and candidates has been weakened, then it is probable that fewer people would be mentioning leaders when probed for their opinions about the parties, despite the fact that candidates have become increasingly salient to the electorate.

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[7] The candidate evaluation data were originally gathered as part of a research project undertaken by Arthur H. Miller, who kindly granted me permission to use them. The dimensions on the party likes/dislikes questions have been constructed precisely according to the subheadings in the election study codebooks. The same is true for the candidate dimensions with the exception of that of general philosophy. Most of the codes in this dimension can be found between 805 and 828 in the 1972 and 1976 codebooks. The distinction between responses on this dimension and those on domestic and foreign policy is that the former deals only with broad comments such as liberal, conservative, humanistic, anti-states' rights, pro-big government, etc.

In working with these responses, one must be careful to compare years in which the leadership situation was as similar as possible. Otherwise the results might simply be due to some exogenous factor such as whether the incumbent was running for reelection or not. The best set of comparisons which can be made are those between 1956 and 1972, when a popular incumbent was seeking reelection, and between 1964 and 1976, when re-

Table 4. A Comparison of the Frequency of Substantive Responses to the Party and Candidate Likes/Dislikes Questions, 1952-1980 (Percent)

	Mentioned for Neither	Mentioned for Party Only	Mentioned for Candidate Only	Mentioned for Both
Domestic Issues				
1952	36.7	53.5	2.7	7.1
1956	49.9	31.8	7.8	10.4
1960	51.7	31.0	7.2	10.1
1964	46.8	15.9	20.5	16.7
1968	54.1	22.0	11.2	12.7
1972	49.6	14.2	21.6	14.7
1976	50.9	19.8	16.6	12.7
1980	46.6	11.0	27.7	14.7
Foreign Issues				
1952	55.6	17.4	16.2	10.8
1956	59.9	12.1	16.8	11.2
1960	56.9	13.1	18.8	11.1
1964	64.3	9.0	19.5	7.2
1968	54.7	13.9	20.0	11.4
1972	46.1	6.6	39.1	8.2
1976	74.3	9.1	11.5	5.1
1980	46.0	3.5	41.6	8.8
Benefits Provided to Groups				
1952	54.0	33.2	4.7	8.1
1956	51.1	32.7	5.3	10.9
1960	56.8	28.3	4.3	10.6
1964	54.4	27.2	7.4	10.9
1968	58.5	25.4	5.5	10.6
1972	54.7	23.8	8.5	12.9
1976	55.0	24.3	9.1	11.6
1980	59.7	28.3	5.2	6.9
General Political Philosophy				
1952	65.1	20.9	8.8	5.2
1956	76.3	15.7	4.9	3.1
1960	68.1	15.9	9.5	6.5
1964	54.6	14.8	16.5	14.1
1968	54.2	19.3	13.7	12.7
1972	62.8	16.7	12.3	8.2
1976	63.6	17.8	10.9	7.7
1980	69.7	13.2	9.1	7.9

Source: National election studies conducted by the University of Michigan's Survey Research Center/Center for Political Studies.

cent mid-term successors to the presidential office sought a term of their own.

These two comparisons support the hypothesis that the party-candidate link has deteriorated. In 1956, 34.4 percent of the respondents mentioned a party figure as a reason for liking or disliking a party while only 19.0 percent did so in 1972. The comparable figures for 1964 and 1976 are 20.4 and 14.5 percent, respectively.[8] The first comparison is particularly interesting, for Eisenhower was a far less partisan figure than Nixon. However, one plausible explanation could be Eisenhower's great personal popularity in 1956. Yet it is not merely positive responses about party leaders which were more prevalent in 1956; dislikes were more frequently mentioned as well. In fact, more people, when asked what they disliked about the Democratic party, mentioned party leaders in 1956 than in 1972 even though Stevenson was a far more popular standard-bearer than McGovern. Regarding the incumbent party, 9.4 percent expressed some dislike of its leaders in 1956 compared to only 3.4 percent in 1972. Although most of the negative comments concerning the Republicans in 1956 were directed at Nixon rather than Eisenhower, it is somewhat unexpected that fewer people would give similar comments about him in 1972—when he was an incumbent president.

In summary, at least a partial answer to the question, "Why the growing neutrality towards the parties?" seems to be that fewer people are translating their likes and dislikes about the candidates and the candidates' stands on specific issues into likes and dislikes about the parties. Future research should concentrate on further examining the linkage of candidates and their stands on the issues to voters' perceptions of the parties over time.

Conclusion

The results presented in this article offer more hope for the revitalization of American political parties than most previous work on the subject. However, one can also extrapolate reasons to be doubtful about such a prospect based on the interpretation of party decline which has been argued here.

Taking the positive side first, the most important new finding is that there has been little increase in the proportion of the population holding negative attitudes towards the parties. It is quite possible that the initial decline in strength of party identification may have been due to the large jump in negativity apparent in 1968. But since that time negative attitudes towards the parties have subsided. Positive attitudes have also continued to decline, but what has increased has been neutrality rather than negativity.

In addition to the fact that parties do not have to overcome largely negative attitudes towards them, it is also encouraging to note that their recovery probably does not hinge on a restoration of trust in government. Like other institutions, political parties are viewed more cynically than in the past. Specifically, more citizens now believe that parties are more interested in votes than in people's opinions. However, this sort of cynicism is apparently not being translated into negativity towards the parties. People may be more skeptical about the motives of parties, but that does not necessarily mean that they dislike parties in general or that they will not identify with a party in the future. This point is supported by the fact that hardly any relationship was found between strength of party identification and trust in government, except for the .11 correlation in 1968. And in a dynamic sense, panel data shows that the two trends are quite independent of one another. Given that the decline of partisanship has been a reflection of growing neutrality instead of negativity on the part of the electorate, such null findings are quite explicable. If negative attitudes about parties were at the root of the rise of independence, then one would expect that other negative attitudes such as cynicism would be related to it, as indeed the case of 1968 appears to show. But, overall, the evidence demonstrates that the decline of parties in the electorate has been more a function of a reduction in salience than an increase in negative attitudes.

However, turning to the reasons to be pessimistic about any revitalization of parties in the electorate, there seems to be little prospect for reversing the trend toward neutrality in the immediate future. One of the most important findings in this article is that the decline of parties in the electorate can be traced back much further than the mid-1960s in terms of party evaluations. The decline of polarized views of the parties and the increase in neutrality is visible throughout the election study time series. Thus it can undoubtedly be considered as a long-term secular trend, and such trends are usually difficult to reverse. In addition, these altered public attitudes have themselves become a major reason why it will be difficult to re-institutionalize political parties.

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[8] In 1980 the comparable figure reached a new low of 12.9 percent. Given that both Carter and Reagan were perceived negatively—i.e., more dislikes than likes were expressed about both—this may be considered as fortunate for the parties. However, as a continuation of a long-term weakening of the party-candidate link in the voters' minds, this development implies that even should popular candidates emerge their popularity is not likely to have much of a positive impact on attitudes towards the parties.

Furthermore, the appearance of a major realigning issue—which is so often considered to be the best hope for reviving the parties—may not have any strengthening effect on partisanship in the electorate. The reason for party decline has not been that people no longer see any important differences between the parties. Indeed, the trend towards neutrality would have been even sharper if the frequency of comments concerning the general philosophies and group benefits offered by the parties had not remained fairly stable. Rather, the problem which the parties must face is that they are considered less relevant in solving the most important domestic and foreign policy issues of the day. In the voters' minds, the parties are losing their association with the candidates and the issues which the candidates claim to stand for. Thus major new issues which arise will probably not help the parties rebuild their base of support unless voters are convinced that the parties can provide a meaningful function which candidates alone cannot.

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The 1928-1936 Partisan Realignment: The Case for the Conversion Hypothesis

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An unresolved question concerning the New Deal realignment is the extent to which the Democratic surge in the vote resulted from either the conversion *of former Republicans or the* mobilization *of newly active voters. Analyzing survey data from the* Literary Digest *straw polls and from early Gallup polls, we find evidence supporting the conversion hypothesis. New voters in 1928, 1932 and 1936 were only slightly more Democratic in their voting behavior than were established voters. Between 1924 and 1936, the vote among established voters was extremely volatile, largely accounting for the Democratic gains. After 1936, however, vote shifts became minimal and party identification had become highly consistent with presidential voting, suggesting a crystallization of the New Deal realignment by the late 1930s rather than a gradual evolution due to generational replacement.*

Between 1928 and 1936 party affiliation among voters in the United States changed dramatically. The predominantly Republican electorate of the 1920s was replaced by a predominantly Democratic electorate during the 1930s. Exactly how this realignment came about, however, remains a matter of some controversy. Two explanations are commonly advanced. The *conversion* hypothesis holds that the partisan change occurred because many Republicans began to identify with the Democratic party as a response to the unique events of the time. This possibility is the most obvious. Sundquist (1973, p. 3) asserts that "in the 1930s, millions of voters shifted their party identification from the Republicans to the Democrats." Similarly, Burnham (1970, p. 6) argues that during realignments "as much as a fifth to a third of the voters" shifted their partisan allegiance. The *mobilization* hypothesis is the second interpretation of the New Deal realignment. According to this view, associated with the authors of *The American Voter* (1960, pp. 153-56), Democratic gains during the New Deal era came largely from a mobilization of new voters previously uninvolved in politics. Converse (1975, p. 140) observes that one can easily "sketch a model of what is going on in this period which requires a proportion of true conversions scarcely more than a percentage or two of the electorate as a whole."

In an imaginative use of survey and aggregate data, Kristi Andersen has in a recent book (1979) advanced a series of arguments which lend considerable support to the mobilization thesis. Andersen's survey analysis is based on the reported partisan conversions of 1952-1972 SRC/CPS respondents and the recalled first presidential votes by 1952 SRC respondents. From these data, Andersen infers that the New Deal realignment resulted from the massive mobilization of previously inactive segments of the electorate into the Democratic ranks. Andersen's aggregate analysis shows that although the Republican *percentage* of the vote declined during the realignment era, the *absolute* number of Republican votes, nationwide and in selected locales, remained fairly constant.

While Andersen's book has generally drawn plaudits for its strong if not definitive support of the mobilization hypothesis (Beck, 1980), there are, nevertheless, serious problems with the data and with the inferences drawn. In particular, the reconstruction of partisanship in the 1920s and early 1930s has been subject to strong criticism. Niemi, Katz and Newman (1980, p. 648) demonstrate that there are "extraordinarily large" errors in reconstructing past partisanship from SRC/CPS surveys. The authors conclude:

> The party identification recall questions used heretofore—and most likely any others that could be designed—are woefully inadequate. By this we mean that responses are unreliable in the technical sense that they are inconsistent across time and that they inaccurately reproduce past partisanship at both the individual and aggregate levels.

Furthermore, Niemi et al. argue, the errors in this reconstruction are biased in support of the mobilization hypothesis in that they underestimate the actual amount of change in partisanship. This point is reinforced in a recent article by Reiter (1980). Blacks and Jews are two groups known to have been Republican in the 1920s and Democratic after the New Deal realignment. However, when Reiter applied Andersen's recall methodol-

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ogy to older black and Jewish SRC/CPS respondents, he found that they overwhelmingly report themselves as having been Democrats during the 1920s—indicating a very serious distortion in recall.

In addition to the problem of bias in respondents' recall of political decisions made decades earlier, it is not even clear that these retrospective accounts are consistent with the findings of surveys conducted during the tail end of the realignment period. Although previously inactive women and immigrants presumably contributed to the New Deal majority, the mobilization argument cannot be correct unless newly eligible *young* voters were also drawn disproportionately to the Democratic party. After examining early Gallup polls, Ladd and Hadley (1978) find that presidential vote decisions in 1936 and 1940 and party identification in 1940 do not vary with the respondents' age. From this relatively direct evidence, the authors conclude (1978, p. 82):

> These surveys conducted during the 1930s and 1940 *neither reveal a New Deal political generation, nor sustain a picture of the young being consistently more supportive than the old of the party of change* [emphasis in original].

Problems exist with Andersen's aggregate-level analysis as well. Consider the inferences that can be drawn from the fact that the Republican vote generally remained constant (in absolute numbers) during the Democratic realignment even in locales, such as big city wards, where the Democratic surge was the strongest. Andersen contends that this increase in the number of Democratic voters coupled with a stable number of Republican voters supports the mobilization hypothesis. Actually, this information is consistent with either the mobilization *or* the conversion hypothesis. As both the number of active voters and the Democratic percentage of vote increases, it is a logical necessity for the Democrats to gain more voters than the Republicans—whatever mix of the two hypotheses happens to be true. But whether or to what extent the Republicans gain votes, lose votes, or stay the same is a direct function of the aggregate-level turnout and the aggregate vote division and *not* the mix of the individual vote decisions that creates these outcomes.[1]

The logic of testing the conversion versus mobilization hypotheses requires individual-level data from the realignment era. But Gallup and other "scientific" pollsters did not begin their efforts until 1936, two elections short for the necessary information to conduct a survey analysis of the vote during the 1928-1936 realignment. There are, however, survey data, albeit of a very tarnished sort, that go back to the 1928 election and include a report on whether the respondent voted four years earlier, and if so, for whom. These data come from the straw polls conducted by the *Literary Digest*. Our purpose in this article is to analyze these data for evidence of voter mobilization and/or conversion to the Democratic party, and thereby throw additional light on the nature of the 1928-1936 realignment.[2] Given the notoriety that surrounds it, the *Digest* poll needs to be discussed in some detail.

The *Literary Digest* Straw Polls

In 1916, *The Literary Digest* began taking presidential "straw polls," basically as a means for

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[1] As a demonstration of how the relative change in the number of Republican voters depends solely on aggregate statistics, consider a hypothetical electoral change from 40 percent Democratic to 60 percent Democratic (i.e., a 50 percent increase) along with a 50 percent increase in the number of voters from 120 to 180. In the first election, the number of Republican votes must be .60 times 120, or 72. In the second election, the number of Republican votes must be .40 times 180, or 72. The number of Republican votes stays constant because the proportional increase in turnout equals the proportional change in the vote percentage. The stability of the Republican vote could result from either mobilization or conversion. As an example, consider first how it could have resulted from the *mobilization* of new voters.

	Election 1		Election 2	
	Dem.	Rep.	Dem.	Rep.
Existing Voters	8	12		
Established Voters	40	60	40	60
New Voters			68	12
Total	48	72	108	72
Percent	(40)	(60)	(60)	(40)

But the same result could obtain by *conversion:*

	Election 1		Election 2	
	Dem.	Rep.	Dem.	Rep.
Existing Voters	8	12		
Established Voters	40	60	40	60
New Voters			48	32
Total	48	72	108	72
Percent	(40)	(60)	(60)	(40)

[2] We are analyzing the data as reported by the *Literary Digest*. We do not have the original data, which presumably has long since been lost or destroyed.

boosting circulation. By 1924, these polls were being conducted nationwide. *The Digest* sent out mail questionnaires to a sample drawn from "every telephone book in the United States, from the rosters of clubs and associations, from city directories, lists of registered voters, classified mail-order and occupational data" (*Literary Digest,* August, 1936,, p. 3). In 1928, 1932 and 1936 the *Digest* asked individuals for both their preference in the current presidential campaign and for whom they voted in the previous election. These returns were cross-tabulated on a national and state-by-state basis and, in 1928, for the cities of Chicago, Philadelphia and New York. New voters were also separated for an independent analysis. Since the poll was basically a publicity scheme, the *Digest* mailed out questionnaires to a large number of people—19 million in 1928, 20 million in 1932 and 10 million in 1936. In each of these years there were over two million responses; thus there are usually more than 10,000 or so individuals in each state sample and around 100,000 for the three cities analyzed separately in 1928. Between 1920 and 1932 the *Digest* correctly predicted the election winner, but the accuracy of the popular vote projections varied. In 1928 the *Digest* overestimated the Hoover vote by 5 percent, and in 1932 it overestimated the Roosevelt vote by about 1 percent. However, in 1936 the *Digest* overestimated the Landon vote by 19.6 percent and predicted (incorrectly) that Landon would defeat Roosevelt in the contest for president. Two years later the *Digest* went out of business.

Given the large bias in the 1936 poll, it is important to discuss with some care the sampling methodology employed by the *Digest.* Shively refers to the sample as basically representing the "attentive middle class." We agree with this description, but not with the assertion that the composition of the samples remained essentially constant over the series of elections (Shively, 1971-72, p. 622). While all *Literary Digest* presidential samples are biased toward the more affluent, it seems likely that the 1936 sample is atypically biased in this direction.

The conventional explanation for the *Digest*'s gross misprediction is that voting polarized along class lines in 1936, with the "haves" voting overwhelmingly for Landon and the "have-nots" overwhelmingly for Roosevelt. Since the *Digest* oversampled the affluent, a strong Republican partisan bias emerged—one which did not exist when class and party were largely unrelated. There can be no doubt that class lines did solidify in 1936, but one can question whether this factor in and of itself was sufficient to introduce a distortion of the magnitude that appeared in the 1936 poll, but did not appear in 1932. While the rise of class voting clearly contributed, other factors unique to 1936 served to further bias the poll in the Republican direction. One little-noticed factor was that the *Digest* changed its sample in 1936. In 1928 and 1932 the *Digest* sent out 20 million mail questionnaires, but in 1936 it sent out only ten million.[3] There is good reason to believe that the reduction of 10 million came basically at the expense of the less affluent, pro-Democratic segments of the population. It is important to reiterate that the *Digest*'s purpose in polling was to stimulate interest in and circulation for the magazine. Therefore, those most likely to be culled would be those least interested in the magazine and its advertisers—i.e., the have-nots. This contention can be supported empirically. The recall by respondents in 1936 of their 1932 vote is strongly biased in favor of Hoover. The recall of the 1932 vote projects Roosevelt with only 48 percent of the vote, whereas the actual 1932 *Digest* poll had predicted Roosevelt the winner with 60 percent of the vote. Thus, the 1936 recall of the 1932 vote *underestimates* Roosevelt's vote by 12 percent, while the 1932 poll *overestimates* Roosevelt's vote by about 1 percent. This recall bias did not occur in 1928 or in 1932, and there is no very plausible reason to believe that something happened in 1936 to make people forget for whom they voted in 1932. Rather, it seems probable that the composition of the sample changed. Evidence for this change can be seen in that in 1928 and 1932 the return rates for the *Digest* poll were 14.6 and 14.3 percent, respectively. But in 1936 the return rate jumped to 22.6 percent. Given what we know about the relationship between socioeconomic status and the return rate for mail surveys, the most likely explanation for this increase is that the sample was more middle-class in 1936 than it was in 1932. The increased bias toward the middle class may also have been aided by the nature of the "protest vote" in 1936. Protest against Hoover in 1932 cut across class lines. Roosevelt was not identified with the social welfare programs that came to characterize the New Deal (he was, for example, calling for a balanced budget in 1932). However, in 1936 the nature of the protest vote changed. It now centered among the affluent who were disenchanted with the directions taken by the New Deal (see Roper, 1936, pp. 58-60).

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[3] Perhaps as a Depression austerity move, the *Digest* used either all or part of the 1932 mailing list for its 1936 sample. According to the *Digest* (5 Sept. 1936, p. 7), "The basics of the 1936 mailing list was the 1932 mailing list." Thus it may be that the 10-million-person sample in 1936 was taken entirely from the 20-million-person dample in 1932. One suspects that those living in the same residences between 1932 and 1936 would be biased toward the middle class—economic circumstances had not forced them to move.

Our argument then is that the 1928 and 1932 *Literary Digest* presidential polls are not as badly biased as the notorious 1936 poll would suggest. The rise of class voting, the change in the sample, and the nature of the 1936 protest vote all operated in a fashion to bias the 1936 poll in a new way. The 1928 and 1932 polls still overrepresent the "attentive middle class," but two points merit emphasis. First, this overrepresentation did not prevent the poll from being reasonably accurate as a predictor of the partisan division in the vote for those years. Second, we know how the sample is biased, and we can take advantage of this information when we evaluate the level of stability and change in the respondents' vote preferences. Given that we can extend a time series back from 1976 to 1928 (and the voters' recall in 1928 of how they voted in 1924) using the *Digest* data, and that a very important question can be addressed through this strategy, we believe that the benefits gained in using the *Digest* data are worth taking the risks involved.

Method

With survey data that include the respondent's recall of his or her previous vote (or nonvote), we can estimate directly the extent of mobilization and conversion among voters sampled by the *Literary Digest*. Evidence regarding mobilization can be obtained by comparing the vote intentions of new voters and established voters. If mobilization is a major explanation for the Democratic gain, then these new voters should be considerably more Democratic in their vote intentions than are established voters. Evidence regarding "conversion" (which, unless we indicate otherwise, means a change from one party to another, reflected in the vote) can be obtained by comparing the vote intention of the established voters with their vote in the previous election, as they recall it.

Although applying this method to the *Digest* data is not without its problems, they are more manageable than might first appear. We have, of course, just argued that reliance on recalls of past political behavior can be highly misleading. However, from an analysis of the 1972-1976 CPS panel data, we find that recalls of presidential votes cast four years earlier are quite accurate. Among 1972 voters in the panel, 93 percent in 1976 correctly reported their 1972 vote (as measured in the 1972 interview). Voters who recall incorrectly tend to err in the direction of partisan consistency, so that reliance upon voter recall slightly underestimates the actual number of vote shifts between 1972 and 1976. Thus it is likely that whatever bias exists in reports of previous votes will be a bias in favor of the mobilization hypothesis, as evidence of conversion will be underreported.

A potentially more serious problem concerns sampling bias. Although estimates from *Digest* samples of how many new voters were mobilized versus how many changed the partisan direction of their vote may be correct as they apply to the "attentive middle class," generalizing from these data to the national electorate is more problematic. But if we take into consideration the effects of the probable sampling biases, we can draw some fairly firm inferences. Remember that the *Digest* sampled atypically white, native-born, middle- to upper-middle-class (and therefore probably older) voters—those with sufficient political interest to complete and return postcard questionnaires. This group is exactly the one we would expect, judging from current literature to be most stable in its voting preferences (Converse, 1962; Dreyer, 1971-1972; Zukin, 1977). Thus self-selection, like the bias in recall, should work *against* the conversion hypothesis. It is also important to remember that the *Digest*'s vote forecasts for 1928 and 1932 were reasonably accurate. That the *Digest* sampled Republicans and Democratic voters in their appropriate proportions does not imply that its samples were representative on all criteria, but it does offer reassurance that the reported vote divisions for new voters and for established voters could not be far from correct. The reason is that for the vote percentage of either subsample to be biased, both subsamples would have to be biased and (implausibly) biased in opposite partisan directions so that the biases would cancel out. For example, if the *Digest* oversampled Democrats among established voters, it must have oversampled Republicans among new voters.

Of course, any observed conversions—by individuals or voters in the aggregate—would be direct manifestations of changes in *voting behavior* only, and by themselves would not necessarily imply a major shift in *party identification.* One possibility, consistent with theory, is that voting behavior changed first, and was later followed by a change in partisanship. Regardless, it seems clear that a rapid shift in party identification did take place. As measured by the two-party vote in House elections, the "normal vote" for the 1918-1928 period was only 42.5 percent Democratic (Converse, 1975, p. 139)—an advantage in favor of the Republicans that more than matches the contemporary Democratic advantage. By 1937, the time of the first survey reading on party identification, the Democrats held a robust lead over the Republicans of 50 to 34 percent (Cantril, 1951, p. 576).

Mobilization of New Voters: The Evidence

Table 1 presents the basic evidence for an evaluation of the mobilization hypothesis. The table displays the vote intentions of new and established voters in the 1928 and 1932 *Literary Digest* surveys and the 1936 to 1976 Gallup and SRC/CPS surveys. In 1928, new voters reported voting 47 percent Democratic (the same as Andersen's recall estimate), 15 percentage points more Democratic than established voters. But in 1932, the year with the largest Democratic gain and the most accurate *Digest* poll, new voters cast ballots only three percentage points more Democratic than established voters. The 1936 Gallup poll shows new voters being slightly *less* Democratic than those who had voted previously.

Of course, the mobilization thesis is based not only on the assumption of disproportionate Democratic voting by new voters but also on the fact that a turnout surge during the realignment period introduced an abnormally large proportion of first-time voters into the electorate. The proportions of each of the various samples who were new voters are shown in the third column of Table 1. The *Literary Digest* undersampled new voters, so the *Digest* proportions shown in Table 1 are probably far too low. But were there unusually large numbers of new voters in the realigning elections? The final column of Table 1 shows for each election the percent increase in the total number of votes from the previous election. For 1932 and 1936 the national turnout increase was in the normal range (compared to elections after 1936), which suggests that new voters made up about 20 percent of the total. (The Gallup estimate of 22 percent new voters in 1936 supports this contention.) Only for 1928 was there an abnormal surge in turnout, on a par with the one later exceptional turnout surge in 1952. During the 1952 surge, 30 percent of those casting ballots were new voters. If we use this 30 percent as a baseline figure for new voters in 1928 and use 20 percent as the baseline for 1932 and 1936, we can make the following computations.[4] Let us assume from our preceding

Table 1. Percent Democratic Vote and Turnout among New and Established Voters, 1928-1976

	Percent Democratic Vote*		Percent New Voters (Survey Estimates)	Percent Turnout Increase or Decrease
	Established Voters	New Voters		
1928 (Digest)	32	47	18	26.5
1932 (Digest)	59	62	14	8.0
1936 (Gallup)	60	59	22	14.8
1940 (Gallup)	51	59	16	9.3
1944 (Gallup)	50	55	14	−3.9
1948 (Gallup)	43	52	23	1.7
1952 (Gallup)	45	49	30	26.1
1956 (Gallup)	42	50	16	0.8
1960 (Gallup)	51	54	24	11.0
1964 (SRC/CPS)	71	78	17	2.6
1968 (SRC/CPS)	45	47	17	3.6
1972 (SRC/CPS)	36	48	22	6.2
1976 (SRC/CPS)	45	69	12	4.1

Sources: Literary Digest 99 (3 Nov. 1928), p. 6; *Literary Digest* 114 (5 Nov. 1932), p. 6; the 1936 to 1960 Gallup data is taken from V. O. Key, Jr., *The Responsible Electorate* (Cambridge, Mass.: Harvard University Press, 1966), p. 24 and p. 43; the 1964-1976 data comes from the SRC/CPS surveys and was made available by the Inter-University Consortium for Political and Social Research, University of Michigan.

*Computed as a percent of the two-party vote.

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[4] The proportions for the 1928 and 1932 electorates that were new voters could be estimated precisely if the exit rates of voters in the previous elections were known. If we can assume that these exit rates are identical to the exit rates for 1936 and 1940, which can be calculated from the turnout increases and proportion of new voters in those years, then

$$\text{Percent Turnout Increase} = \frac{\text{New Voters} + \text{Established Voters}}{\text{Exiting Voters} + \text{Established Voters}} \times 100.$$

According to this equation, the exit rates for 1936 and 1940 are computed to be 8 percent in 1936 and 10 percent in 1940. Plugging the average of these two values into the equation for the earlier elections yields estimates of 29 percent new voters in 1928 and 17 percent new voters in 1932. Thus the estimates in the text of 30 analysis that the true differential between the voting behavior of new and established voters is 15 percent more Democratic in 1928, 3 percent more Democratic in 1932, and 1 percent more Republican (from Gallup) in 1936. Multiplying terms, we get .30 times 15 percent for 1928, .20 times 3 percent for 1932, and .20 times a —1 percent for 1936. These computations add up to 4.9 percent as the net increase in the Democratic vote attributable to the entry of new voters. Thus even if we doubled our estimate of the number of new voters entering the electorate, the net increase would still fall far short of the actual nationwide Democratic gain between 1924 and 1936 of 27.7 percent.

These calculations assume, of course, that the vote differential between new and established voters in the *Digest* sample is identical to the vote differential within the electorate at large. Since the *Digest* undersampled new voters, could not the modest partisan differences between new and established voters be the result of a systematic understampling of new Democrats and a consequent oversampling of new Republican voters? This possibility is highly unlikely for the simple reason that the bias in the *Digest* procedure did not prevent reasonably accurate estimates of the Democratic percentage of the vote in 1928 and 1932. Given that the estimates of the total percent Democratic in these two elections are correct, then for the *Digest* to have *under*estimated the Democratic proportions among new voters, it must have *over*estimated the Democratic proportions among established voters. Logically it can be the other way. Since there is no reason to believe that the *Digest* oversampled established Democrats at the expense of established Republicans (the reverse in fact seems more plausible), the percent of new voters that are Democrats and Republicans must be essentially correct.

Voter Conversion: The Evidence

Assuming the mobilization hypothesis accounts for only a small part of the New Deal Democratic surge, it is then almost tautologically true that the major source of the Democratic gain came from vote switching among established voters. It does not necessarily follow, however, that one would find such a gain among established voters in the *Literary Digest* samples. As we have already indicated, the biases in the *Digest* poll should work against the conversion hypothesis. If, however, established voters surge to the Democratic party in the face of these biases, it would be strong evidence favoring the conversion hypothesis.

Table 2 displays, for established voters, the relevant data from the 1928 and 1932 *Literary Digest* polls and the 1936 Gallup survey. The data are presented as total percentage rather than row or column percentage since vote shifts are usually analyzed in terms of proportions of the entire electorate. For each election, the table shows the proportion of established voters who switched their votes in each partisan direction and the proportion who were "stand pat" Republican and "stand pat" Democratic voters. Adding cells "b" and "c" shows that 26 percent switched their vote between 1924 and 1928. Twenty-nine percent switched their votes between 1928 and 1932, while between 1932 and 1936 the figure drops to 19 percent (and continues to drop in the two following elections).[5] Thus there is a major shifting of voters at exactly the expected time. Of interest here is the net advantage that accrues to the Democrats as a result of these shifts.

In 1928, despite the high level of vote switching, the net Democratic gain is only 2 percent (or 4 percent when conversions from 1924 minor party candidates are taken into account). This is less than the actual national swing of 6.4 percent, but we saw earlier that mobilization was a factor in the modest 1928 Democratic gain. Nevertheless, the pattern of considerable movement with a small net partisan change among established voters is what one would expect at the beginning of a conversion-based realignment. Party attachments are weakening and the new coalitions are beginning to form.

The 1932 data present a different picture: 1928-1932 vote switches were one-sided, creating a Democratic gain of 26 percent based on these conversions. This conversion-based Democratic gain among established voters is even greater than the national swing of the two-party vote (18 percent Democratic) between 1928 and 1932. We now have two bits of evidence which suggest that the major Democratic increase of 1932 came mainly from the conversion of established voters rather than the mobilization of new voters. Earlier we saw that new voters were not disproportionately Democratic in 1932, a figure not likely affected by the *Digest*'s biased sampling procedures. We now find direct evidence of substantial conversion among established voters in the face of biases which should serve to attenuate such conversion.

In 1936 the national vote swing was 3.3 percent

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percent new voters in 1928 and 20 percent new voters in 1932 may be slightly generous.

[5] According to Gallup data, reported in Key (1966, p. 19), 17 percent switched their votes between 1936 and 1940 and 11 percent switched their votes between 1940 and 1944. See also Figure 1.

Table 2. Cross-Tabulation of Vote by Reported Vote Four Years Earlier for the 1928, 1932 and 1936 Elections (Percent)

	1924 Reported Vote		
	Democratic (Davis)	Republican (Coolidge)	Total*
1928 Vote			
Democratic (Smith)	20	14	34
Republican (Hoover)	12	55	67
	32	69	101
N = 2,204,765 (*Literary Digest*)			

	1928 Reported Vote		
	Democratic (Smith)	Republican (Hoover)	Total
1932 Vote			
Democratic (Roosevelt)	33	26	59
Republican (Hoover)	3	38	41
	36	64	100
N = 2,458,664 (*Literary Digest*)			

	1932 Reported Vote		
	Democratic (Roosevelt)	Republican (Hoover)	Total
1936 Vote			
Democratic (Roosevelt)	53	7	60
Republican (Landon)	12	29	41
	65	36	100
N = 3,780 (Gallup)			

Sources: Literary Digest 99 (3 Nov. 1928), p. 6; *Literary Digest* 114 (5 Nov. 1932), p. 6; the Gallup data is taken from V. O. Key, Jr., *The Responsible Electorate* (Cambridge, Mass.: Harvard University Press, 1966), p. 43.

*Percents may not total to 100 due to rounding.

Democratic. However, the Gallup data for 1936 show a 5 percent net *Republican* gain among established voters. The explanation for this inconsistency is that Gallup slightly underestimated the total 1936 Democratic vote while the 1932 recall figures (among established voters) slightly overestimate the actual 1932 Democratic vote. It seems that the combination of a rise in class voting and a mild SES sampling bias caused the undersampling of 1936 Democrats. A true 1936 Republican gain from conversion is unlikely because, as we saw, established voters were more Democratic than were new voters in Gallup's 1936 sample. But the exact figure here is not important. What is important is that there was no major Republican gain based on conversion to offset the 26 percent net conversion gain made by the Democrats between 1928 and 1932 (plus an additional 2 percent gain made between 1924 and 1928).

A question that must be addressed concerns how much may be inferred about the voting behavior of the general population based on our analysis above. The rival interpretation is that the conversion explanation works for the subpopulation of likely *Digest* respondents, but not for others. If the *Digest* data contain a combination of biases that mask a true mobilization effect for the general electorate, these biases must be that new Republicans were sampled more heavily than new Democrats *and* that converts (vote switchers) were sampled more heavily than stand-pat established voters. The former bias, as we saw, could not be present unless Democrats were sampled more heavily than Republicans among established voters. An oversampling of Republican-to-Democratic converts could, however, account nicely for an oversampling of established Democratic voters, although there is strong reason to believe that converts would be *under*represented in the *Digest*'s "attentive middle class" sample. But let us momentarily suspend judgment on the grounds that behavior may be different during realignments.

If the mobilization hypothesis is correct, but its existence is masked by bias in the *Digest*'s sample, the following rival scenario must be true: the conversion hypothesis worked only for the attentive middle class. Members of this group who had voted previously defect in large numbers to Democratic candidates. Members of this group who had not voted previously cast their ballots only slightly more Democratic than the electorate as a whole. Meanwhile, the mobilization hypothesis worked for the "underclass" (those unlikely to get into the *Digest*'s sample). *Established* voters from the underclass continued to vote as they had in the past (generally Republican). However, new voters from the underclass surged to Roosevelt in response to the events of the time.

The difficulty with this scenario is that if it were true the Democratic party had to have drawn more support within the established electorate from the attentive middle class than from the underclass. In other words, if sampling bias distorted the *Digest*'s evidence for the conversion hypothesis, there had to have existed negative class voting (the wealthier established electorate voting more Democratic than the working-class established electorate). This possibility seems sufficiently implausible that we feel safe in rejecting it. More likely, considerable conversion must have taken place among the electorate as a whole.

The 1928 Vote in the Large Cities

If any group of new voters made a major contribution to the realignment Democratic surge, we would expect this group to be city dwellers who before 1928 were inactive. A major thrust of Andersen's mobilization argument is that 1928 saw substantial increases in both turnout and Democratic voting in large cities, particularly in white ethnic precincts. The logical inference is that hitherto unmobilized voters accounted for created the Democratic gains. Fortunately, the *Literary Digest* reported breakdowns of the 1928 straw poll for each of the three largest cities—New York, Chicago, and Philadelphia. An additional reason for separate analysis of the big-city data is that for these three cities the *Digest* sent mail ballots to all registered voters rather than following their usual procedure of haphazardly sampling from such lists as telephone directories or automobile registrations. Therefore, any socioeconomic or other bias among large-city respondents can be due only to differential return rates and not from a bias in the initial sampling lists. According to the *Digest* data, only 18 percent of the big-city respondents were new voters—clearly an underestimate which suggests that these individuals were less likely to return their straw-poll ballots than established voters. But the *Digest* estimates of the Democratic vote in these three cities were, nevertheless, correct within 5 percent of the actual vote.

For the three cities, the 1924 to 1928 turnout surge averaged an unusually high 45 percent, and the Democratic increase in the two-party vote averaged 22 percent. To see whether the new Democratic voters were disproportionately drawn from the electorate previously inactive, we can examine the *Digest* data, presented in Table 3.

New voters in the *Digest* sample are only a few percentage points more Democratic in their 1928 voting than are established voters, giving little support to the mobilization hypothesis. For each of the cities there is, however, a substantial amount of vote switching—even more than shown in Table 2 for the entire 1928 electorate. But unlike the national data which show a net 1928 Democratic gain of only 2 percent due to conversion, the big-city data show net Democratic gains of 12 percent in New York, 20 percent in Chicago, and 27 percent in Philadelphia *due to conversion.* On average, these Democratic conversion-based gains are within 3 percentage points of the actual net Democratic gain for the three cities in question. Thus in these large cities, where the mobilization thesis should receive its strongest support, our data show that a more persuasive case can be made for the conversion hypothesis.

The above interpretation may appear somewhat incautious because of the possibility that a truly large Democratic surge by new voters in the large cities was masked by the *Digest*'s biased sampling procedure. We have acknowledged that the *Digest* undersampled new voters. But the crucial question concerns the partisan division of their vote contrasted to that of the established electorate. The *Digest* data show new voters to be only slightly more Democratic than established voters, and it seems highly likely that this figure is correct. The argument made earlier on this point in reference to the national data applies to the city data as well. Given the accuracy of the marginals, if new Democrats were undersampled more than new Republicans, then established Republicans must have been undersampled more than established Democrats—a happenstance that seems highly implausible. Moreover, one must somehow account for the Democratic conversion among the *Digest*'s atypically middle-class voters.

Vote Switching: A Closer Look

We have seen that the New Deal Democratic surge can be largely explained by the conversion of established voters. If this behavior contradicts the predictions of existing theory, it is because established voters presumably have strong party identifications which immunize them from changes in partisan behavior. Party attachments may, however, already have begun to break down by the advent of the realignment. As evidence,

Table 3. 1928 Presidential Vote by Reported 1924 Presidential Vote for *Literary Digest* Respondents in New York, Chicago and Philadelphia (Percent)

	1924 Reported Vote			
1928 Vote	Democratic (Davis)	Republican (Coolidge)	Row Totals (Established Voters)	New Voters
New York (N = 198,681)				
Democratic (Smith)	35	20	55	62
Republican (Hoover)	8	37	45	38
	43	57	100 (83)*	100 (17)
Chicago (N = 137,946)				
Democratic (Smith)	17	24	41	42
Republican (Hoover)	4	55	59	58
	21	79	100 (82)	100 (18)
Philadelphia (N = 96,370)				
Democratic (Smith)	10	30	40	44
Republican (Hoover)	3	56	60	56
	13	87	100 (82)	100 (18)

Source: Literary Digest 99 (3 Nov. 1928), p. 7.

*The figures in parentheses are the proportions of the respective samples that are new and established voters.

from Table 2 we saw what appears to be a considerable amount of vote switching during the realignment elections. To ascertain whether this amount of vote switching is anomalous, we shall use as a standard the extent of vote switching in more recent elections.

Appropriate for this analysis are column percentages, which control for the net swing (the total percentages in the cells of Table 2 are in part a function of the marginal counts). Accordingly, Figure 1 shows the defection rates for each party from surveys dating between 1928 and 1976. The vertical axis indicates the percent of those voting Republican in the previous election who "defected" and voted Democratic in the next election. The horizontal axis indicates the reverse.

In general, a high number of Republican defections tend to be associated with low numbers of Democratic defections and vice versa. This is the pattern of vote switching one would expect as a response to short-term partisan forces favoring a single candidate. We can also observe that the amount of vote switching varies from one election to the next. We have fitted two symmetrical functions to the data in Figure 1. One line fits the observations from 1940 to 1956—the period of partisan consolidation following the New Deal realignment. Another line fits the observations from 1960 to 1976—the more recent period when party attachments seem to be losing their authority.[6] As expected, vote switches are more frequent during the recent period than during the 1940-1956 period.

Turning to the crucial realignment elections, we see that the 1928, 1932 and 1936 observations all fall beyond even the 1960-1976 line. There is, during this period, more "against the partisan tide" movement than in the other eras—indicating a sorting-out of voter coalitions.[7] We need, in addition, to keep in mind that the bias in the *Digest* sample should work to suppress the extent of switching. Nevertheless, individual vote decisions from one election to the next were at least as unstable for the realignment period's "attentive middle class" as for voters generally during the

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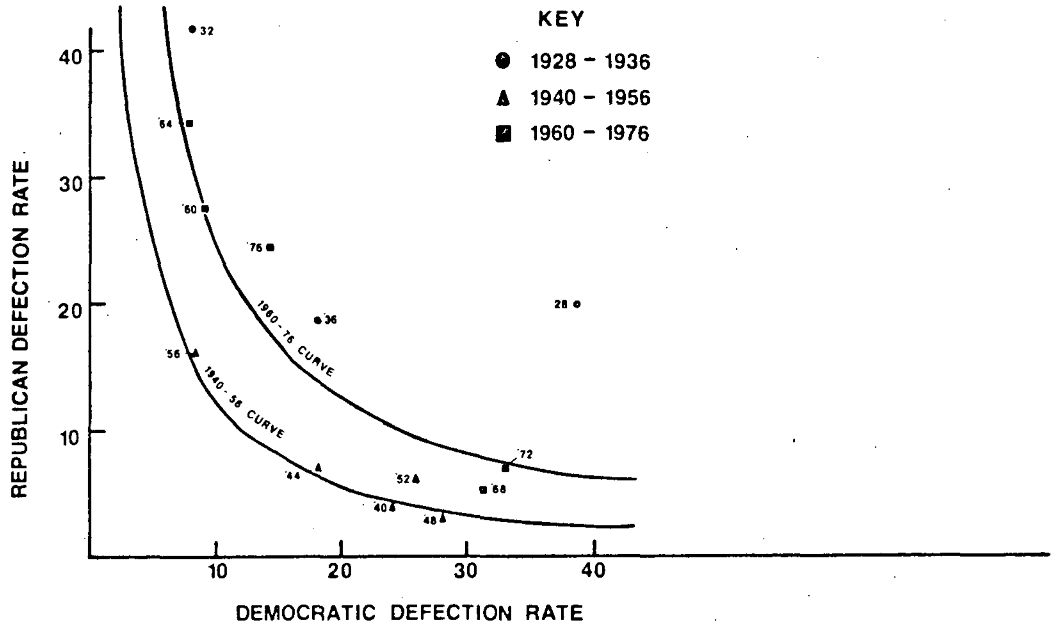
[6] For both 1940-1956 and 1960-1976, the defection rates show a close fit with that predicted by a function whereby the product of the Republican and Democratic defection rates equals a constant. For each period, this function was computed as the mean product of the two defection rates. For 1940-1956, the mean product is 118 percent; for 1960-1976 it is 254 percent.

[7] Of all the election results displayed in Figure 1, the clear outlier is 1928. In part, the massive amount of vote switching in 1928 can be accounted for by reactions to Al Smith's religion. But many of these vote switches went against the grain of the religious issue. Although Davis to Hoover switches were most frequent in the mainly Protestant states, the Coolidge to Smith switches varied little with the state's religion. For example, in the four nonsouthern states that were less than 10 percent Catholic, where 45 percent of the Davis voters switched to Hoover (N = 63,929), 18 percent of the Coolidge voters switched to Smith (N = 96,688). Thus, almost one out of five Protestant Coolidge voters apparently supported Smith in 1928.



Sources: Literary Digest 99 (3 Nov. 1928), p. 6; *Literary Digest* 114 (5 Nov. 1932), p. 6; the 1936 to 1960 Gallup data is taken from V. O. Key, Jr., *The Responsible Electorate* (Cambridge, Mass.: Harvard University Press, 1966), p. 24 and p. 43; the 1964-1976 data comes from the SRC/CPS surveys and was made available by the Inter-University Consortium for Political and Social Research, University of Michigan.

Figure 1. Defections among Republicans and Defections among Democrats (1928-1976)

current "dealignment" era. This movement indicates that the ties of party were weak. For a sizable portion of the established electorate to shift to the Democratic party it is helpful if the pull of party identification is not too strong.

Mobilization, Conversion and Party Identification

We have argued that the Democratic vote surge of the New Deal era resulted from vote switching among established voters. It is unclear, however, whether these conversions in presidential voting were accompanied by conversions in party identification. Conceivably, the mobilization hypothesis may still be valid in the context of *party identification,* with the Democratic gain in partisanship resulting essentially from the gradual introduction of new Democrats into the active electorate.

One must recognize, however, that the Democratic gain in party identification was itself quite rapid. Sundquist (1973) notes that the big surge in Democratic *registration* (the one available surrogate for party identification) occurred in the mid-1930s. Averaged across three states (California, Oregon and Pennsylvania), Democratic registration increased from about 30 percent in 1932 to 50 percent in 1936. We also have evidence from the first Gallup reading of party identification in 1937, which shows a Democratic to Republican ratio of 50 to 34 percent (Cantril, 1951, p. 576). Clearly, by the mid-1930s, more people called themselves Democrats than Republicans, in sharp contrast to what we imagine surveys would have shown a few years earlier. Could this rapid Democratic gain in party identification have resulted from new voters calling themselves Democratic by a lopsided margin while established pre-realignment voters persisted in their Republican identification even as they voted Democratic? More plausible, we believe, is the explanation that established voters began to identify with the Democratic party, although perhaps with some delay following their "conversion" in voting behavior.

One could well imagine someone doing survey research during the realignment period and attributing the voter movement in 1928 and 1932 to short-term forces—perhaps a reaction to Smith's Catholicism in 1928 and a protest against Hoover in 1932. But after voting Democratic in a series of elections, many one-time Republicans may have begun to think of themselves as Democrats. This contention is supported by recent analysis of the

1972-1976 CPS panel (Markus and Converse, 1979; Howell, 1980) which shows that presidential voting does exert a lagged effect on party identification. Markus and Converse (1979, p. 1061) assert that "the cumulative effects of a series of votes running counter to an individual's prior party ties might well lead to a conversion of party orientations at the individual level."

According to this logic, by 1936 many of the previous Republican-to-Democratic vote converts would have become Democrats in party identification. The mobilization hypothesis, however, offers a different expectation for 1936: that the Democratic landslide resulted from short-term defections by Republican identifiers with only relatively new voters being strongly Democratic in *both* voting and party identification. These competing expectations can be tested. Presented in Table 4 is the relationship between party identification and the recalled 1936 vote from a March 1937 Gallup survey, which is the earliest survey asking respondents about party identification. These data show striking support for the conversion hypothesis: only 5 percent of the votes cast in 1936 were by either Republicans or Democrats voting counter to their partisanship. In fact, party voting in 1936 is higher than in any of the 1952-1978 SRC/CPS presidential or *congressional* election surveys.[8] Furthermore, if one divides the 1937 sample into those first eligible to vote in 1932 or after and those first eligible in 1928 or before, there is little difference in either their partisanship or their candidate preference. Young voters were slightly more likely to vote Democratic and prefer the Democratic party label (as expected), but even older voters were preponderantly Democratic in both voting and party identification—rarely combining their Democratic vote with a Republican identification. By 1936 the realignment was essentially complete. Party identification had become consistent with voting behavior.

If our analysis is correct, why did people change their party identification during the New Deal realignment era, but not, for example, in response to the strong electoral forces of the past two decades? One of the attractions of the mobilization hypothesis is that even in the face of declines in the directional strength of contemporary partisanship, voters' party identification is quite stable (Converse and Markus, 1979). Given this stability, the notion that partisan realignments are driven by the mobilization of new voters and not conversion of established voters is understandably ap-

Table 4. Party Identification Cross-Tabulated by 1936 Presidential Vote (Percent)

1936 Vote	Party Identification Democratic	Independent*	Republican	Row Totals
All Voters (N = 2,382)				
Roosevelt	51	11	4	66
Landon	1	5	28	34
	52	16	32	100
Voters 21-29 years old (N = 413)				
Roosevelt	55	12	4	71
Landon	1	6	22	29
	56	18	26	100
Voters aged 30 years and older (N = 1,950)				
Roosevelt	50	11	4	65
Landon	1	5	29	35
	51	16	33	100

Source: American Institute of Public Opinion, Survey #72, March 1937.

*Includes Socialists and "no opinion" responses to the party identification question.

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[8] For a comparison with Table 4, see Table 2 of Mann and Wolfinger (1980, p. 620). This table shows a greater proportion of party defections in 1956-1978 congressional races than found among 1936 presidential voters. The table shows slightly greater proportions of party-line voters than found in 1936, but this can be accounted for by Mann's and Wolfinger's inclusion of independent leaners with party identifiers. Even counting independent leaners as partisans, we find less party-line voting in each national House election since 1964 than occurred for president in 1936.

pealing. But it is important to remember that the unique catalyst of the New Deal realignment was the permanent change in the policy alignments of the two parties at the elite level. Recent electorate tides such as those that occurred in 1964 and 1972 were also the result of party shifts at the elite level, but these were essentially of the short-term variety. One can easily imagine that if either the 1964 or 1972 alignments had become permanent (such as the capture of the Democratic party by the McGovern wing in 1972), considerable partisan conversion would have been the result. Similarly, one can imagine that in the 1930s voters changed their partisanship in response to the new elite policy alignments.

Conclusion

As is often said, individual-level data is preferable to aggregate-level data when one wishes to generalize to individual-level behavior. In this article we have analyzed what appear to be the only individual-level voting data from the early part of the New Deal realignment. Several conclusions emerge. First, new voters in the *Literary Digest* polls were only slightly more Democratic than were established voters. It therefore seems unlikely that the surge in Democratic voting resulted from the mobilization of a large number of previously inert Democratic partisans. Rather, the *Digest* data indicate that much of the Democratic gain came from established Republican voters who switched and began to vote Democratic. These traditional Republican voters may not have been converted immediately to Democratic partisans, and in fact there was sufficient movement by voters in both partisan directions during the realignment to suggest weak party attachments. But by 1937 the electorate had become predominantly Democratic—of that there can be no doubt. What seems to have happened is that following the early years of the New Deal, party attachments crystallized in response to presidential vote decisions. Much of the New Deal partisan realignment may be explained as the conversion of former Republicans to the Democratic party by virtue of attitudinal changes resulting from changes in voting behavior.

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Understanding the Tax Revolt: Eight Explanations

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Numerous explanations of the tax revolt have been offered since California's adoption of Proposition 13 in 1978. Unfortunately, many of these explanations remain untested or have been tested inappropriately, and the explanations are often jumbled together in a fashion that precludes theoretical clarity. We extract eight explanations from the literature, each of which assumes that the tax revolt is a systematic national phenomenon that is a function of individual-level social, economic, and political factors. Having tested these explanations by means of a discriminant analysis of data from the 1978 American National Election Study, we find little empirical corroboration for any of them. This leads us to consider two alternative research programs, one of which interprets the tax revolt within a symbolic politics framework.

The tax revolt swept across the country with astonishing speed in the wake of California's favorable vote on Proposition 13 in 1978. In 1978 alone, 17 states conducted initiatives on state and/or local taxation (Mikesell, 1979), and by 1980 38 states had moved to reduce or at least stabilize taxes (Peirce, 1980). The revolt is obviously important to government officials who must now provide services under severe revenue constraints. More importantly, however, the tax revolt raises serious questions about the workings of representative government. As Shapiro, Puryear, and Ross argue (1979, p. 1), it is troublesome to learn that "within a stable democracy as ours, the government could find itself so out of line with the desires of the governed." It is not surprising, then, that Paul Samuelson considers California's Proposition 13 to be perhaps the most important political-economic event of the 1970s in this country (Levine, 1980, p. 123).

Academia's response to the tax revolt has been unusually swift, as evidenced by the appearance of nearly 100 articles between 1978 and 1980. However, a review of this large and growing literature reveals some serious flaws in these attempts to explain the tax revolt. Most importantly, the literature is a theoretical mishmash. With rare exceptions, there has been little attempt to sort out the sources of the revolt, and the various explanations that have been offered for the most part remain untested or inappropriately tested. At this point, little can be said about the validity of the untested explanations. For those that have been tested, however, two problems stand out. First, most explanations of the sources of support for the tax revolt focus on *individual-level* economic, social, and political factors. Yet many purported tests of these explanations employ precinct-, county-, or state-level data on voting in tax initiatives, and thereby run afoul of the ecological fallacy; even if we know, for example, that support for tax limitation is lower in jurisdictions with large nonwhite populations, we cannot conclude that whites are more favorable to tax limitations than nonwhites are. Second, most studies have gauged support for tax limitation in terms of votes pro or con in initiatives in which two separate tax limitation measures were considered concurently. There is evidence that voters tend to view such dual choices as alternatives to one another (Citrin, 1979, p. 126). Thus, voters could have supported a particular proposal because they favored it on the basis of its merits *or* because they wished to avoid the imposition of an even more drastic alternative; they could have opposed a particular proposal because they rejected tax limitation in general *or* because they preferred a different method of limitation. In any case, individuals with very different motivations would have been thrown together as either for or against tax limitation, even though they may have had very little in common. The analytic literature is also weak in an epistemological sense. Lakatos (1970) argues that theoretical progress can be made only if different theories within a research program are tested *against* one another. To this point, however, the norm has been to test only a single, partial model of the sources of support for the tax revolt, with minimal attention given to evaluating different models or considering a more

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The data used in this study were made available by the Inter-University Consortium for Political and Social Research. The data for the American National Election Study, 1978, were originally collected by the Center for Political Studies of the Institute for Social Research, the University of Michigan, under a grant from the National Science Foundation. Neither the original collectors of the data nor the Consortium bears any responsibility for the analyses or interpretations presented here.

comprehensive explanation; the result has been a proliferation of isolated explanations without a concomitant expansion of explanatory content.

In short, the study of the tax revolt displays many of the characteristics of what Lakatos calls an "immature" research program, which is understandable given the recent and very sudden emergence of the tax revolt. This article attempts to redirect the tax revolt literature. In the first section, we lay out eight different explanations of the revolt. These are not the only explanations that have been put forward, but these eight clearly dominate the literature. They also comprise a single research program in that they all look upon the tax revolt as a systematic national phenomenon, understandable in terms of individual-level economic, social, and political factors. These eight explanations are used as the bases of a comprehensive model of support for tax limitation—a model which we test in our second section via discriminant analysis of data from the 1978 American National Election Study. In the third and final section, the findings of this analysis lead us to offer several suggestions for further study of the tax revolt.

Explanations of the Tax Revolt

The tax revolt literature suggests a number of explanations of why people support tax limitation measures—explanations which are usually jumbled together in a fashion that creates confusion about just what is being said concerning the sources of support for the tax revolt. In this section, we draw eight such explanations from the literature, which, though presented separately, are in most cases not mutually exclusive. They focus on a wide range of factors. The first emphasizes people's rational self-interest; the next three pertain to evaluations of the tax system; the fifth concerns personal economic anxieties; the next two deal with basic political orientations; and the last centers on levels of information about government and public finance. We present these explanations in approximate order of the frequency with which they have been cited in the literature.

The Self-Interest Explanation. The most familiar explanation of the tax revolt is based on a narrowly defined rationality model in which the individual's demand for government taxes and expenditures is seen as a function of self-interest, as inferred from the individual's demographic characteristics (e.g., race, income, or home ownership). The California and Michigan tax revolt referenda have been frequently studied using self-interest models (Mariotti, 1978; Courant, Gramlich, and Rubinfeld, 1979, 1980; Citrin, 1979; M. Levy, 1979; Magaddino et al., 1980).

In light of the long history of successful applications of the self-interest model, it may come as a surprise to learn that analyses examining the characteristics of supporters for tax limitation have generally produced very unimpressive results. With the exception of the studies by Levy and Magaddino et al., multivariate analyses which employ demogrpahic predictors have been unable to account for much variance in support for tax limitation, and most of the coefficients run in the wrong direction and/or are statistically insignificant. This has led to some pessimistic assessments of the utility of self-interest models in explaining the tax revolt (Mariotti, 1978, p. 24; Citrin, 1979, p. 127). But while previous studies have produced little evidence supporting the self-interest explanation, their poor showing may be attributable to methodological problems rather than to shortcomings of the explanation itself. All except Mariotti have focused on situations in which people were voting on two different tax limitation proposals at the same time, a choice structure that could easily confound tax revolt voting behavior. More seriously, most of these studies use aggregate data to test individual level self-interest hypotheses.

The Tax Level Explanation. This interpretation views the tax revolt as an attempt to trim dramatically what is perceived to be a bloated government—a necessary, if unprecedented, response to Leviathan government (Buchanan, 1979; Brennan and Buchanan, 1979). In this sense, tax-limitation or tax-reduction initiatives may prove a rare outlet for venting grievances against national, state, and local taxes.

The limited empirical work on this explanation has produced ambiguous results. Attiyeh and Engle (1979), who examined the county-level vote for Proposition 13, found that support for the proposition was higher in high-expenditure counties. Here again, we confront a series level-of-analysis problem, but there is also some individual-level evidence which corroborates Attiyeh's and Engle's interpretation. According to pollster Mervin Field (1978, pp. 4-5), the primary bond that held supporters of Proposition 13 together was their conviction that taxes were too high. But other surveys call this conclusion into question. It has been shown, for example, that Californians anticipated only slight cuts in service levels as a result of the passage of Proposition 13 (Citrin, 1979; Mushkin, 1979; Hebert and Bingham, 1979). Thus considerable uncertainty remains about the empirical validity of this explanation, but again it is uncertain whether these ambiguous findings reflect substantive problems with the tax level explanation or methodological difficulties in testing it.

The Tax Efficiency Explanation. This explanation focuses on the relative rather than absolute level of taxation and suggests that the tax revolt is a reaction to perceptions of rampant waste and inefficiency in the public sector (Mushkin, 1979; Lucier, 1980). Taxes are considered too high *relative to benefits received,* and it is argued that the motivation underlying tax limitation is to put government on a "fiscal diet" which will redress this imbalance.

The evidence bearing on this explanation is mostly indirect. The steady increase in the percentage of Americans who feel that government officials waste their tax money is taken as evidence that concern about efficiency underlies voting for tax revolt referenda (Hebert and Bingham, 1980, p. 146; Citrin, 1979, p. 116). However, while these findings are consistent with the efficiency explanations, they fail to relate perceptions of inefficiency directly to voter support of tax revolt referenda. A limited amount of survey evidence bears more directly on the tax efficiency explanation. One suggestive finding comes from a California poll taken in June 1978, which uncovered a strong relationship between support for Proposition 13 and perceptions of waste in government (Citrin, 1978; see also Scott, Grasmick, and Eckert, 1981). However, the fact that this survey was taken shortly after the vote on Proposition 13 leaves open the possibility that this finding reflects merely ex post facto rationalization and thus undermines any confidence in the only evidence which speaks directly to the validity of the tax efficiency explanation (Citrin, 1978, p. 120).

The Tax Distribution Explanation. This infrequently cited explanation suggests that support for tax limitation is premised not on the level of taxes or the uses to which tax revenues are put, but rather on the perceived inequities of the tax system. For instance, Shapiro et al. (1979, p. 4) argues that "Proposition 13 was an expression, by the property owner-voter, of dissatisfaction with bearing a growing share of the public financial responsibility." This concern with the distributive impact of taxation is thought to extend well beyond the property tax: many tax analysts have argued that the revolt represents a reaction against taxes in general, spurred on by people's feelings that they are bearing more than their fair share of the tax load. Unfortunately, this explanation has yet to be put to an empirical test.

The Economic Pinch Explanation. This explanation sidesteps issues of public spending and taxation and concentrates instead on anxiety over the condition of the economy in general and personal finances in particular. Boskin (1979, p. 42) presents this view most forcefully when he contends that "the recent movements to limit taxes and government spending are deeply rooted in the lack of private economic progress in the last half decade." Inflation and recession can trigger several of the causal mechanisms cited in other explanations of the tax revolt, but for the most part, this explanation emphasizes anxiety over personal finances rather than the objective economic impact of recession and inflation.

Little research has been conducted on this explanation. The only relevant study we know of is one by Courant et al. (1980), who included a "gloom" variable in a generally self-interest-based analysis of Michigan's 1978 vote on the Headlee and Tisch amendments; they found that economic pessimists were more likely than optimists to support cuts and/or limits on public spending and taxation.

The Political Ideology Interpretation. According to this explanation, support for the tax revolt is more a matter of ideology than of demography or economics. Clearly, this explanation overlaps with several others: it seems plausible that conservatives might be more likely than liberals to view the level of taxation and the scope of government as excessive and to see government's use of taxes as wasteful and inefficient. But at its core, this explanation views the tax revolt as a symbolic challenge to 50 years of New Deal Liberalism (Musgrave, 1978, p. 701).

Previous findings concerning this "new conservatism" explanation are very inconsistent: Citrin (1979, p. 126) and Field (1978, p. 6) both found that liberal political ideology and Democratic party identification did tend to reduce the likelihood of an individual supporting the tax revolt, while Courant et al. (1980, p. 9) and a CBS News/*New York Times* poll uncovered no relationship at all (*Public Opinion,* 1978, p. 32). But as noted previously, the effect of multiple tax revolt choices in the states studied by these analysts weakens their findings. Despite the attractiveness of this explanation to political scientists, further testing is clearly required.

The Political Disaffection Explanation. This explanation views the tax revolt as a reflection of declining confidence in government rather than a function of the resurgence of conservative views about the proper role of government. Negative feelings about government—brought on by factors as varied as potholes in the streets, welfare cheating, and corruption in high places (Musgrave, 1978, p. 701)—carry far beyond a simple concern with efficiency in service delivery or a lingering resentment of taxes that are considered inordinately high. Such feelings go deeper, ultimately raising the questions of confidence in government and alienation from the political system. It is particularly unfortunate, given the implications of this explanation, that it has never been directly tested.

The Information Explanation. The central idea of this final explanation is that support for the tax revolt reflects a certain naivete, or, perhaps more charitably, a lack of information about government—an idea suggested by the oft-mentioned divergence between the promises made by tax limitation proponents and the actual policy effects of Proposition 13 (*Consumer Reports,* 1979). This performance gap has led some to conclude that only those who are grossly ill-informed about government in general and public finance in particular could favor Proposition 13-style tax limitation (McCaffrey and Bowman, 1978, p. 530). The explanation is seldom made in anything but the most offhand manner and has yet to be tested.

Testing the Explanations

Data and Methods. As indicated, the empirical portion of this article is based on data that the University of Michigan Center for Political Studies collected for the American National Election Study in 1978. As is true of virtually any secondary analysis, some issues that are of central concern to us were not dealt with in very great detail in the survey. On the other hand, the 1978 election study data set does give us a large, representative, nationwide sample to work with; moreover, we have found *at least* one indicator appropriate for testing each explanation, and in certain cases we have been able to employ multiple indicators of key concepts.

TAXCUT, the dependent variable, is based on the answers respondents gave when asked whether they would "vote for or against a measure similar to Proposition 13" if they were given a chance to do so. Before answering this question, respondents had been told that Proposition 13 reduced property taxes in California by more than half and that its opponents claimed that it would force local communities to cut back on services. Of the 2,304 respondents, 1,740 (75.5 percent) said they had heard or read about Proposition 13. Some 75.1 percent of the respondents were willing to take a stand for or against a Proposition 13-style tax limitation, while the remaining 24.9 percent made ambivalent replies (4.0 percent), answered "Don't know" (19.5 percent), or gave no answer at all (1.4 percent). Of the 1,700 who gave unambiguous responses, more than seven in ten (71.3 percent) were in favor of a measure like Proposition 13.

In the analysis that follows, we use a total of 25 different predictors of support for property tax limitation. Detailed information concerning these variables is given in Table 1, but a few brief comments regarding some of them are also in order.

First, we selected the demographic variables for inclusion from among those which had performed most impressively in prior tests of the self-interest explanation: in all, we used eight such variables. Second, TOOHIGH, WASTE, and UNFAIR—test variables for the tax level, tax efficiency, and tax distribution explanations, respectively—all pertain to respondents' feelings about taxes in general rather than the property tax in particular. Although we would have preferred to include some items that tap feelings about property taxes in particular, the tax variables that we were able to use are appropriate for testing the three tax system explanations, all of which focus on feelings about taxes in general rather than, or in addition to, specific dissatisfaction with property taxes.

Third, as additional test variables for the tax level explanation, we employ estimates of each state's "perceived property taxes" and "total perceived taxes" (Levy, 1979), coded as state-level contextual variables for each respondent in the 1978 ANES survey. Levy calculated "perceived property taxes" by multiplying total property tax collections by the proportion of all assessed valuation in the state composed of residential property; he contends that this provides a rough estimate of the property tax burden residents of each state face. "Total perceived taxes," which give a rough estimate of the total tax burden in each state, are figured as the sum of perceived property taxes, state and local personal income taxes, and state and local sales taxes, all divided by personal income. We are not entirely satisfied with this measure as an indicator of dissatisfaction with the level of taxation; it is simply the best we could do in the absence of survey items on this topic.

Fourth, we have treated three explanatory factors as having only nominal-level properties, although they are customarily considered ordinal or even interval scales. The first of these is party identification. Because Weisberg (1980) has shown that "Independent" is not a middle category between "Republican" and "Democrat," we created three dummy variables indicating whether each respondent was a Republican, a Democrat, or an Independent. In multivariate models, one of an exhaustive set of dummy variables is excluded from the prediction equation, with the excluded variable assuming the role of the reference category for the dummies that remain. We also converted respondents' assessments of their present and future financial situations into sets of dummy variables. This decision was based on indications that favorable and unfavorable economic assessments may have asymmetrical political consequences (Kernell, 1977).

Table 1. Descriptions of Variables Used to Test Eight Explanations of the Tax Revolt

Explanation	Variable	Description
	TAXCUT	0 = "against," 1 = "for," in response to "If you had the chance, would you vote for or against a measure similar to Proposition 13 in your state?"
Self-Interest	RACE	0 = nonwhite, 1 = white.
	GENDER	0 = female, 1 = male.
	AGE	Age in years.
	INCOME	22-point scale of 1977 pre-tax family income.
	EDUCATION	Number of years of school completed.
	CLASS	0 = working, 1 = middle, based on self-identification.
	SIZE	Population of community, logged due to distribution.
	OWNERSHIP	0 = renter, 1 = homeowner.
Tax Level	TOOHIGH	0 = agree, 1 = disagree, with the statement that "Despite all the complaints, federal taxes really are *not* too high considering the services the government provides for people."
	PERPROPERTY	"Perceived property taxes," as defined by Levy (1979): total property tax collections multiplied by the proportion of all assessed valuation in the state composed of residential property. State score coded for each state resident.
	PERTOTAL	"Total perceived taxes," as defined by F. Levy (1979): sum of "perceived" property taxes, state and local personal income taxes, and state and local sales taxes, all divided by personal income. State score coded for each state resident.
Tax Efficiency	WASTE	0 = "not much" or "some," 1 = "a lot," as answers to "Do you think that people in the government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it?"
Tax Distribution	UNFAIR	0 = disagree, 1 = agree, with the statement "I feel that most people who have a higher income than I do, manage to get away with paying less than their fair share of taxes."
Political Ideology	CONSERVATIVE	Score on a 7-point scale running from "extremely liberal" to "extremely conservative," based on self-identifications.
	REPUBLICAN	0 = non-Republican, 1 = Republican, based on self-identifications.
	DEMOCRAT	0 = non-Democrat, 1 = Democrat, based on self-identifications.
Political Disaffection	INEFFICACY	0 = agree, 1 = disagree, with the statement that "Sometimes politics and government seem so complicated that a person like me can't really understand what's going on."
	OUTEFFICACY	0-4 scale summarizing responses to the following items: "People like me don't have any say about what the government does"; "I don't think public officials care much what people like me think"; "Generally speaking, those we elect to Congress in Washington lose touch with the people pretty quickly"; and "Parties are only interested in people's votes but not in their opinions."

(continued on next page)

Table 1. (continued)

Explanation	Variable	Description
Political Disaffection (cont.)	POLITICAL TRUST	0-5 scale summarizing responses to the following items, dichotomized as by Miller (1974): "How much of the time do you think you can trust the government in Washington to do what is right?" "Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?" "Do you feel that almost all of the people running the government are smart people, or do you think that quite a few of them don't seem to know what they are doing?" "Do you think that quite a few of the people running the government are crooked, not very many are, or do you think hardly any of them are crooked crooked?" "Do you think that (Watergate, sex scandals, and corruption) are just the result of what individual politicians have done or is it because there's something more seriously wrong with government in general and the way it operates?"
	RESPONSIVE	0-8 scale summarizing responses to the following items: "Over the years, how much attention do you feel the government pays to what the people think when it decides what to do?" "How much do you feel that political parties help to make the government pay attention to what the people think?" "And how much do you feel that having elections makes the government pay attention to what the people think?" "How much attention do you think more Congressmen pay to the people who elect them when they decide what to do in Congress?"
Economic Pinch	BETTERNOW	0 = "same" or "worse," 1 = "better," in response to "Would you say that you (and your family living here) are better off or worse off financially than you were a year ago?"
	WORSENOW	0 = "same" or "better," 1 = "worse," in response to the same question.
	OPTIMIST	0 = "same" or "worse." 1 = "better off," in response to "Now looking ahead—do you think that a year from now you (and your family living here) will be better off financially, or worse off, or just about the same as now?"
	PESSIMIST	0 = "same" or "better," 1 = "worse off," in response to the same question.
Information	INFORMATION	1-5 scale used by interviewers to rate the respondent's "general level of information about politics and public affairs," with 5 indicating the highest level of information and 1 the lowest.

Source: Compiled by the authors from the University of Michigan Survey Research Center American National Election Study for 1978; Frank Levy, "On Understanding Proposition 13," *The Public Interest* (1979): 66-89; Arthur H. Miller, "Political Issues and Trust in Government," *American Political Science Review* 68 (1974): 951-72.

To account for the possibility that a similar logic could underlie people's attitudes toward tax limitations (with those who feel economically pinched being particularly negative toward the tax system while those whose situations are improving are not particularly positive about taxes), we coded two sets of three dummy variables apiece for the items in question, and selected two from each set for use in the analysis.

Fifth, we employ two separate efficacy variables (INEFFICACY and OUTEFFICACY) because of indications that the items normally used to create a single efficacy scale are in fact two-dimensional (see, for example, McPherson, Welch, and Clark, 1977). It is generally agreed that these two dimensions relate to evaluations of the self as a political actor ("internal" or input efficacy) and the responsiveness of the political system ("external" or output efficacy). However, no consensus has yet emerged concerning the manner in which the six efficacy items should be combined into separate scales. After considering several possibilities, we adopted the technique suggested by Craig (1980), in which four items are used to tap output efficacy, one is used to measure input efficacy, and the remaining item is scrapped because of its ambiguity and its low correlations with the other items.

Finally, the item used to create the WASTE variable performs double duty in the analysis. It serves as the test variable for the tax efficiency explanation and it is also one component of the composite TRUST variable. In this light, it should come as no great surprise that there is a strong association between WASTE and TRUST ($r = .624$), for we are dealing with what amounts to a part-whole correlation rather than a correlation between independently defined measures.

We have, then, a categorical dependent variable and a set of independent variables that are dichotomous or can safely be treated as interval. In this situation, discriminant analysis is the appropriate statistical technique. The point of discriminant analysis is to distinguish between the groups that comprise the categories of the dependent variable on the basis of a discriminant function, the best linear combination of the independent variables. There are two different ways of judging the predictive power of the discriminant function. The first of these is the canonical correlation, which when squared has an explained variance interpretation. The second is the percentage of cases correctly classified, based only on knowledge of respondents' values on the discriminating variables. For the individual predictors, standardized discriminant function coefficients are perhaps the best-known gauges of relative predictive power. These coefficients are analogous to beta weights in regression analysis and are subject to the same limitations. Most importantly, they can be strongly affected by multicollinearity, making it difficult or impossible to gauge the relative impacts of the various predictors; this could be a serious problem, for example, where variables as closely interrelated as WASTE and TRUST are used in the same analysis. For this reason, Klecka (1980) recommends that analysis focus on the total structure coefficient for each predictor rather than on the standardized discriminant function coefficients. Total structure coefficients are simply product-moment correlations between each independent variable and the discriminant function; a high structure coefficient means that a particular variable conveys nearly the same information as the function itself does, while a low structure coefficient means that a particular variable has very little in common with the combination of variables that best discriminates between categories of the dependent variable. The discriminant results reported below are for an additive combination of predictors. We have also experimented with an extensive set of nonadditive models designed to test for interactive and cumulative effects. In no case, however, did the nonadditive models perform any better than the much simpler additive models; hence our reliance on the latter.

Findings. We will first present the discriminant results that bear on the *relative* discriminatory power of the 25 predictors and, by extension, the relative predictive power of the eight different explanations of the tax revolt. Then we will consider the *overall* discriminatory power of the 25-predictor model, and, by extension, the issue of whether support for property tax limitation can be adequately understood in terms of the types of individual-level factors that are referred to in the eight explanations. Table 2 summarizes our findings in both respects, showing the total structure coefficient for each variable (and, for reference purposes, the simple correlation between each variable and TAXCUT), along with the canonical correlation and the percent correctly classified for the overall discriminant model.

Of the 25 predictors, only one—OUTEFFICACY, with a structure coefficient of -.555—truly stands out in terms of its ability to discriminate between supporters and opponents of property tax limitation. Clearly the best predictor of support for tax limitation, then, is the feeling that one is cut off from the political decision-making process. This same general pattern emerges, albeit less strongly, for the remaining political disaffection variables: support for property tax limitation was greatest among those who were most cynical about political leadership, who were most likely to look upon political institutions as being unresponsive to their needs, and who were most dubious of their own political effectiveness. These results provide some measure of empirical support for the idea that pro-tax limitation sentiments reflect a deeper political disillusionment which is directed against government institutions and authorities.

Indicators associated with two of the three tax-based explanations also perform somewhat more impressively than the remaining predictors. TOOHIGH and WASTE are two of the three best predictors to emerge from the discriminant analysis—findings which provide some corroboration

Table 2. Discriminant Analysis Results

Predictor	Total Structure Coefficient	Simple Correlation
RACE	.259	.062
GENDER	.048	.011
AGE	.048	.012
INCOME	–.059	–.017
EDUCATION	–.325	–.080
CLASS	–.070	–.018
SIZE	–.243	–.058
OWNERSHIP	.097	.022
TOOHIGH	.382	.094
PERPROPERTY	.200	.050
PERTOTAL	.135	.034
WASTE	.406	.099
UNFAIR	.189	.047
CONSERVATIVE	.352	.086
REPUBLICAN	.055	.014
DEMOCRAT	–.385	–.094
INEFFICACY	–.275	–.067
OUTEFFICACY	–.555	–.137
TRUST	–.349	–.087
RESPONSIVE	–.164	–.044
BETTERNOW	.014	.002
WORSENOW	–.106	–.024
OPTIMIST	.042	.009
PESSIMIST	–.035	–.008
INFORMATION	–.207	–.051

Source: Computed from data from the University of Michigan Survey Research Center American National Election Study for 1978.

Note: Canonical correlation .243
% correctly classified 61.5

for the tax level explanation and the tax efficiency explanation, respectively. Political ideology, as indexed by the CONSERVATIVE scale, is another one of the strongest predictors used in this analysis, indicating that to some extent the controversy over tax limitation conforms to the traditional left-right cleavage line. The structure coefficients also reveal that Republicans were not unusually supportive of tax limitation, but that Democrats were with some degree of regularity opposed to it.

The remaining predictors perform much less impressively. Of the eight demographic variables introduced in conjunction with the self-interest explanation, only EDUCATION, RACE, and SIZE display total structure coefficients that are appreciably greater than zero. Thus there was only a very slight tendency for home owners to be more favorable to property tax limitation than renters were, and GENDER, AGE, and CLASS perform even more poorly than HOMEOWNER. The negative coefficient for EDUCATION indicates that, just as in the Courant et al. (1979) study, more highly educated people were *less* supportive of property tax limitation. Another finding consistent with some of the studies cited earlier, was that opposition to property tax limitation was greater in larger communities and among nonwhites, but these differences were not very strong. The same can be said for the INFORMATION variables: those respondents who were rated as most lacking in political information were oriented most positively to tax limitation; but here again, the relationship is at best modest. Finally, the structure coefficients associated with BETTERNOW and WORSENOW, OPTIMIST and PESSIMIST are uniformly negligible, providing no empirical support whatever for the economic pinch explanation.

In sum, although no one explanation stands out above all the rest, the political disaffection, tax level, tax efficiency, and political ideology explanations possess a discriminatory power that is markedly superior to that of the remaining explanations. However, the primary message of Table 2 concerns not the relative discriminatory power of the various predictors but rather their overall ability—or, more accurately, their overall *inability*—to distinguish between supporters and opponents of tax limitation. In this regard, we see first that the correlation between TAXCUT and its *best* bivariate predictor, OUTEFFICACY, is only —.137 and that the bulk of the simple correlations are clustered very close to zero. Nor does considering the predictors jointly in the mullti-variate analysis improve matters much. The 25-predictor model does not permit anything approaching accurate discrimination between tax limitation's supporters and opponents. The correct classification percentage of 61.5 is not much better than the 50 percent correct classification that we could have achieved on the basis of simple random assignment. Even more tellingly, the canonical correlation of .243 for the 25-predictor model means that considered jointly, the 25 predictors account for a grand total of only 5.9 percent of the variance in support for property tax limitation. Thus, while certain explanations obviously perform better than others, none—considered singly or jointly—performs very well.

Discussion

Our findings point very clearly toward the conclusion that support for property tax limitation cannot be adequately understood on the basis of variables suggested by the eight different explanations we have considered. How can we account for the poor showing of these explanations?

One possible interpretation of the unimpressive performance of the eight explanations of the dominant research program is methodological in nature. It could be argued that the variables we have considered may have a greater impact on actual voting in property tax initiatives than on how people say they might vote in a tax limitation initiative if given the chance to do so. The idea here is that confronting a concrete policy choice may increase the saliency of property taxes, causing potential voters to become better informed about this issue, to ponder the alternatives more extensively, and to calculate their self-interest more carefully. This interpretation is at first plausible but ultimately unconvincing, because what it predicts—clear-cut support and opposition patterns in property tax initiatives—is simply not borne out by the facts. As we noted in our first section, previous researchers have failed to turn up any individual-level independent variables that consistently affect voting decisions in property tax initiatives. Our findings, then, are very much in keeping with what has emerged in studies of voting in tax initiatives. And unlike the few individual-level studies that have been conducted on initiative voting, our findings cannot be explained away due to the confounding effects of multiple choices on simultaneous tax limitation propositions.

Another possibility, also methodological, is that some of the indicators we have employed as predictors of support for property tax limitation are at fault. Of the indicators listed in Table 1, this criticism seems most applicable to predictors used in conjunction with the self-interest explanation. More direct measures of self-interest have on occasion produced better results: the studies of Courant, Gramlich, and Rubinfeld (1979, 1980) in Michigan found sharp differences in tax attitudes between public and private employees, and Hansen (1980) found that people who depended on government transfer payments were more willing to pay taxes than others not so situated. Membership in certain demographic groups—the indicator of self-interest used here and throughout the literature—is much less satisfactory, for it is at best "an insensitive index of the personal impact of a policy issue" (Sears et al., 1980, p. 3). Accordingly, the problem may lie less in the self-interest explanation per se than in the difficulties involved in trying to tap into this explanation empirically.

However, a more substantive interpretation of the poor performance of the self-interest explanation would focus on the fact that most people do not evaluate tax cuts in terms of their own self-interest. According to a Gallup poll conducted in January 1978, most people support property tax limitation even though most also believe that it will be others who benefit most from such limitation (Gallup, 1979, pp. 78-79). If tax limitation is usually looked upon as something that primarily helps "the other guy," then the failure of the self-interest explanation is hardly surprising.

Thus we do not believe that the poor performance of the eight explanations is a function of the testing procedures we have employed. Another possibility is that the dominant research program itself is flawed. Despite their many differences, the eight explanations share two key assumptions—that the tax revolt is a systematic national phenomenon and that it is a function of individual-level social, economic, and political factors—which tie them together as a single research program. While much less frequently cited, two alternative research programs can be identified in the literature, each of which modifies one of these assumptions and each of which could account for the null results presented here.

The first alternative research program argues that it is unrealistic to consider the tax revolt a systematic national phenomenon. From this perspective, each state tax revolt is *sui generis*—a function of the specific characteristics of the state in which it occurs (Levy, 1979; Lucier, 1980; Hebert and Bingham, 1979). Indeed, specific events and characteristics of the states do seem to have influenced voting behavior in tax revolt referenda; for example, Michigan's long-standing urban-rural political cleavage and California's recent sharp improvement in property tax assessment practices and rapid increase in housing values probably did affect the outcomes of the initiatives that were conducted in those states. On balance, however, we are not persuaded by this view. In the first place, it strains credulity to look upon the near-simultaneous occurrence of tax revolts in 38 states as sheer coincidence. Moreover, there is abundant evidence that the tax revolt is in fact a nationwide phenomenon. National surveys have consistently revealed that public dissatisfaction with taxes in general and property taxes in particular is not restricted to any one state or region (see, for example, Citrin, 1979, p. 115). We would certainly encourage other researchers to continue experimenting with state- and/or local-level contextual variables like the "perceived tax" measure we have used here. Moreover, if comparable state-level data were available, it would be most instructive to decompose the variance in attitudes toward taxes into national and state components. But beyond this, we would view any move to achieve an understanding of the tax revolt by launching a series of one-state case studies as a step backwards.

The second alternative research program rejects the assumption that the tax revolt is a function of individual-level political, social, and economic characteristics, and instead conceives of the revolt as an expressive or symbolic social movement. To borrow a distinction originally made by Berelson, Lazarsfeld, and McPhee (1954, p. 184), tax limitation, like some of the other great controversies of the last decade, may be a "style issue" as opposed to a "position issue." Style issues involve self-expression of an indirect, projective kind rather than expression of immediate self-interest; they are related to questions of taste, lifestyle, and even morality rather than money and material power; and they are more deliberately "created" and more evanescent than position issues, which arise out of socioeconomic conditions and persist over the long run. This approach has rarely been applied to the tax revolt, with the sole exceptions of an interpretive essay by Sears (1978) and a work by DeCanio (1979) which draws parallels between the tax revolt and the free silver movement of 1896.

The distinction between style and position issues puts the failure of the eight explanations we have considered in a light which, we feel, can help us better understand the origins of the tax revolt. Pollster Louis Harris (1978, p. 1) reminds us, "Most Americans have been unhappy about taxes for a long time." Why, then, did tax limitation so suddenly emerge as a political issue in the late 1970s? We believe that the key to answering this question is that although opinions supportive of tax limitation had long been widespread, these sentiments had never been *mobilized* on a mass basis. Moreover, it is now clear that such sentiments can be mobilized almost literally overnight. In California, for example, Proposition 13 carried 65 percent of the referendum vote, but just two months before the election only 27 percent of those surveyed in a California poll said they were going to support it (Field, 1978, p. 5). Thus widespread disenchantment with taxes was almost certainly a necessary condition for the tax revolt, but it was by no means a sufficient condition. That disenchantment had to be activated and channelized toward the achievement of a particular policy goal. DeCanio (1979) draws special attention to the role of leadership in the free silver movement, suggesting that it was the charismatic leadership of William Jennings Bryan on a highly symbolic issue which finally crystallized agrarian reform. The crusty charisma of Howard Jarvis becomes central to understanding the recent tax revolt from this perspective; his antipathy toward discussing details and his emphasis on such symbols as "lazy bureaucrats" and "lying politicians" should be viewed as key elements in the origin and diffusion of the tax revolt. When coupled with the prevailing public frustration over a number of tax-related issues, the effect of a charismatic leader spearheading a massive organizational effort provides a sufficient condition for the tax revolt.

We believe that the symbolic politics interpretation of the tax revolt goes a long way toward explaining the inadequacies of the eight explanations considered here. More importantly, if the tax revolt is primarily, but not exclusively, a style issue, then different people support or oppose it for different reasons; such a mixed motivational base would mean that one person's overriding concerns are relatively inconsequential for another. Obviously, this sort of situation can put a severe limitation on predictive power: different motives could be functional substitutes for one another rather than combining in a fashion that can readily be handled in a statistical model. Moreover, even among those for whom the motivation is largely expressive, the symbolic meaning of tax limitation may not be widely shared. Rather, tax limitation may be similar in this regard to the Vietnam War and the energy crisis, which according to Sears, Hensler, and Speer (1979, p. 381) "failed to evoke very clear and consensual meaning to Americans. Hence they were not as easily related to symbols which would evoke, on a common, mass basis, the residues of earlier socialization." Sears (1978, p. 1) explicitly singles out the diffuse symbolic meaning of the tax revolt in his interpretation of Proposition 13. He concludes that Proposition 13 resembled "Peter Finch's litany in the film *Network*, 'I'm mad as hell and I'm not going to take it any more!' " But Sears goes on to note, "Most voters do not seem to have a clear sense of what the 'it' is" (Sears, 1978, p. 6).

It is understandable, then, how the "social movement" character of the tax revolt could upset the predictions made by the eight explanations we have considered. Many people who have personal characteristics or attitudes which predispose them to favor tax limitation, as predicted by one or more of the eight explanations, may not have been mobilized by the tax revolt movement. Others who have far weaker predispositions in the direction of tax limitation may by their web of personal and group affiliations have been enlisted into the movement. Finally, those who were mobilized may have been acting out of a variety of different motives. Accordingly, as the movement gained momentum across the country, it may well have obliterated any major economic, social, and political distinctions between supporters and opponents of tax limitation.

Finally, the symbolic politics interpretation leads us to suggest that the tax revolt will be of relatively brief duration. Sears (1978) correctly points out that the "real" issues underlying the tax revolt are those implicit or explicit in the eight explanations we considered earlier (e.g., inflation, unemployment, alienation and unresponsiveness), and that tax revolt referenda bear only slightly on the resolution of many of these issues. In a similar vein, DeCanio notes (1979, p. 59) that "the demand for 'free silver' . . . bore only the most obscure relationship to the complaints articulated by the farmers" in the last third of the nineteenth century. The tax revolt movement, then, will likely break down as attention is refocused on these core issues; tax politics will revert to position politics and will again be dominated by the dynamics of such issues.

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Patterns of Adoption of Tort Law Innovations: An Application of Diffusion Theory to Judicial Doctrines

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Social scientists have given increasing attention to the diffusion of policy innovations among the American states, focusing on the legislative and administrative sectors. This study is an effort to expand our understanding of policy diffusion by analyzing the diffusion of 23 innovative tort doctrines among state court systems between 1876 and 1975. This analysis examines the innovativeness of state judicial systems, the correlates of innovativeness, and the pattern of diffusion. The findings suggest that the diffusion of judicial doctrines is a very different process from the diffusion of legislation. A major reason for the difference appears to be the courts' dependence on litigants to provide opportunities for innovation.

The diffusion of innovations has been a concern of social scientists for over half a century (Rogers and Shoemaker, 1971). Political scientists began to study this phenomenon relatively late, but by now they have produced a considerable body of research on innovation (see Klingman, 1980; and Downs and Mohr, 1979). Beginning with Jack Walker's pioneering examination (1969) of state adoptions of 88 types of legislation in a variety of policy areas, political scientists have been particularly concerned with the diffusion of innovations among the American states.

Except for Glick's focus on modernization of court structures (1981), state-level diffusion studies have dealt exclusively with legislative or administrative innovations. There have been no analyses of the diffusion of new judicial doctrines; this article is a first inquiry into that subject. We have two reasons for conducting such a study. First, we are interested in the diffusion of judicial innovations as a significant process in itself. Second, we think exploration of this heretofore uninvestigated area can add to our knowledge about the diffusion of innovations in general and of new public policies in particular.

Our examination of doctrinal innovation will have two concerns. The first is innovativeness as a characteristic of institutions and the factors which shape it. On this subject we are particularly interested in the view of scholars such as Walker (1969) that organizations will have fairly consistent patterns of innovation across issues. This view is challenged by those who argue that the adoption of innovations is largely issue-specific rather than a general predisposition of adopters (Gray, 1973, pp. 1183-85; Menzel and Feller, 1977). Our analysis can inform this debate by providing evidence from an arena that has not yet been studied. We also will examine the relationship between legislative and judicial innovativeness in the states and the impact of states' characteristics on innovation by their courts.

Our second concern is the pattern of diffusion of policy innovations across the states. We will seek to determine whether judicial doctrines diffuse largely on a regional basis, as Walker (1969) and Foster (1978) found for legislation, or essentially on a national basis, as Gray found (1973). We also will examine the speed of diffusion. These inquiries can provide insights about communication networks within the legal system and about the readiness of judges to adopt innovations.

The Innovations

This study concerns the diffusion of 23 plaintiff-oriented tort doctrines among the state court systems in the 1876-1975 period. The doctrines are listed in the appendix. Space precludes discussion

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of the particular doctrines or their adoption histories, but a detailed account of some exemplary doctrines can be found in Prosser (1960, 1966) and in Canon and Jaros (1979, pp. 971-74).

Tort Law. By focusing on tort law, we sacrificed some capacity to generalize about the diffusion of innovations in the judiciary. However, we felt it desirable to focus our study narrowly because of the possibility that the diffusion process varies considerably across policy areas. Moreover, by comparing innovations within a single area we could provide a strong test of the degree of idiosyncracy in judicial innovation.

We chose tort law for two reasons. First, it has undergone enormous development over the last century. As a result, we were able to find a reasonably large number of doctrinal innovations scattered over a long period. In contrast, doctrine has been much more static in other traditional areas of law such as property law and contract law. Second, we preferred to focus on interaction among the states without contamination from federal action. While federal laws or decisions have affected many diffusion patterns at the state and local levels (Welch and Thompson, 1980), tort law has developed virtually without federal guidance or interference.

All 23 doctrines that we analyze are plaintiff-oriented. That is, they improve the legal position of plaintiffs by such means as creating new rights, easing burdens of proof, and eliminating or weakening certain defenses. Both the plaintiffs who benefit from the doctrines and the defendants who are disadvantaged constitute fairly distinct groups in society. The plaintiffs are usually individuals who have suffered physical injury. By contrast, the defendants are often business corporations, government agencies, or other institutions. When this is not the case, an insurance carrier is often the real if not the nominal defendant.

The choice of plaintiff-oriented doctrines was virtually dictated by the direction of change in tort law over the last 100 years. For much of the nineteenth century, innovations in tort were largely defendant-oriented, aimed at protecting the accumulation of capital for use in industrial development (Friedman and Ladinsky, 1967, pp. 51-58). But subsequently courts—along with other agencies—became more concerned with the social costs of the industrial revolution for injured workers and consumers. As a result, they fashioned doctrines shifting these costs to the more solvent elements of society. The study of tort doctrines favoring plaintiffs also facilitates comparisons with legislative innovation, because tort innovations are similar in ideological direction and often in subject matter to many of those Walker studied (1969, p. 899).

Selection of Doctrines. We began with the common definition of an innovation as a program or policy which is new to the adopting unit, regardless of how old the idea may be or where it originated (Walker, 1969, p. 881). While courts make new law regularly in the course of their work of resolving new factual situations, we limited our concern to those changes which involved major departures from past policies and which sooner or later had a significant impact on the real world. We identified these with the aid of legal encyclopedias, treatises, and casebooks, particularly Prosser and Wade (1971). Of those so identified, our ultimate selections were based upon several criteria.

The first was the scope of the adoption. To be included, an innovation had to be adopted in a minimum of 18 states and could not be explicitly rejected, following the first adoption, in as many states as had adopted it. In other words, it had to be an emergent or dominant doctrine in the law while being diffused. Second, we excluded doctrines in whose development state legislatures played a large role. The relatively few tort law innovations that were adopted by the legislature or were rejected legislatively without prior judicial adoption in more than eight states were not considered. The third criterion was temporal. Doctrines were excluded if a significant part of their adoption process occurred prior to 1876.

Identification of Adoptions. Our identification of adoptions of doctrines was fairly straightforward. We examined the system of regional reports of state court opinions published by the West company for potential adoptions. We considered an adoption to have occurred whenever a state court said or implied that the doctrine was now the law in that state. We focused only on the initial adoption and did not consider subsequent "disadoptions" by the same court or legislative action to override the adoption. Such occurrences, however, were infrequent. We included partial adoptions (e.g., adoptions made with reservations or adoptions of only one element of a doctrine) so long as they constituted significant departures from the previous state of the law.

The vast majority of the adoptions came from the states' highest courts, which we will label "supreme courts." A few came from lower courts.[1] A lower court adoption was not included if the supreme court superseded the decision through either adoption or rejection of the doctrine in the next five years, or if a court of equal authority in the state explicitly refused to follow the doctrine within that five-year period.[2]

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[1] Usually such adoptions were made by intermediate appellate courts. However, trial court opinions are published in a few northeastern states. We included trial court adoptions in these states because if uncontradicted they had authority in their state and became part of the nationally disseminated body of doctrine.

Data and Analysis: Innovativeness

Rankings of the States. We created a composite innovation score for each state based on the timing of its courts' adoptions of the 23 individual doctrines. For each doctrine each state was given a score corresponding to the proportion of the "adoption period" that remained when that state adopted the doctrine; the adoption period begins with the first adoption of a doctrine and ends one year after its last adoption. For a doctrine whose last "counted" adoption occurred in 1975, that year was treated as the year of last adoption. Thus the first adopter would receive a score of 1.000 and the last adopter a score slightly above .000. Nonadopters received scores of .000. This formula is similar to that used by Walker (1969, pp. 882-83).[3] Where the state courts were preempted by federal court adoption or state legislative action, the state was not given a score for that doctrine. The composite score for each state is simply the mean of the individual scores it received for each doctrine. These are shown in Table 1.

The rankings are interesting. It is not surprising that states such as Minnesota and California rank high, because they have reputations for progressivism and innovativeness. Similarly, some low-ranking states such as West Virginia and New Mexico have the opposite reputation. The low rankings of states with small populations, such as Alaska and Wyoming, also might have been expected. But the rankings of many states do not comport very well with conventional wisdom about innovativeness—either in general or as it relates to judicial doctrines. It is almost shocking to see Texas, Kentucky, and Louisiana among the top ten and Massachusetts near the bottom.

Because changing conditions over time in the states may bring about new patterns of innovativeness in judicial decisions, rankings for a century-long period may obscure as much as they show. Consequently, we created separate innovation scores for the early and late segments of our study period by dividing the doctrines into two groups. One consists of 9 doctrines whose median

Table 1. State Innovation Scores for 23 Plaintiff-Oriented Doctrinal Innovations in Tort Law

1.	Minnesota	.509
2.	Texas	.458
3.	Kentucky	.457
4.	Washington	.433
5.	California	.419
6.	Missouri	.400
7.	Louisiana	.388
8.	New Jersey	.375
9.	Connecticut	.374
10.	Oklahoma	.360
11.	Illinois	.354
12.	Tennessee	.353
13.	Pennsylvania	.343
14.	Michigan	.341
15.	Iowa	.339
16.	Ohio	.335
17.	Oregon	.332
18.	Wisconsin	.331
19.	North Carolina	.323
20.	Georgia	.320
21.	South Carolina	.309
22.	New York	.307
23.	Kansas	.295
24.	Mississippi	.293
25.	Indiana	.288
26.	Colorado	.284
27.	New Hampshire	.258
28.	Florida	.251
29.	Alabama	.237
30.5	Arkansas	.229
30.5	South Dakota	.229
32.	Nebraska	.227
33.	Arizona	.226
34.	Montana	.216
35.	Maryland	.202
36.	Delaware	.198
37.	North Dakota	.189
38.	Idaho	.173
39.	Utah	.172
40.	West Virginia	.159
41.	Rhode Island	.147
42.	Virginia	.112
43.	New Mexico	.108
44.	Nevada	.105
45.	Massachusetts	.096
46.	Alaska	.078
47.5	Maine	.074
47.5	Vermont	.074
49.	Hawaii	.053
50.	Wyoming	.019

Source: Collected and compiled by the authors.

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[2] No evidence indicates that any state court system was precluded from adopting or seriously delayed in its ability to adopt these plaintiff-oriented tort doctrines by jurisdictional requirements or appellate structure. For various reasons a few state courts could not adopt a particular doctrine; we excluded these states from consideration for that doctrine.

[3] Walker's procedure differed slightly from ours in that he gave higher scores to the late adopters (although he reversed this procedure in computing the summary innovation scores) and he gave the last adopter the same score as non-adopters rather than a slightly "better" score.

adoption date occurred between 1902 and 1938 (the pre-World War II group); the other consists of 14 doctrines with median adoption dates between 1950 and 1970 (the postwar group). Table 2 shows the time scores and rankings for each of these sets of doctrines and shows the states' changes in rank from the prewar to the postwar period.

There is some similarity between the two sets of scores, as indicated by the correlation between

Table 2. Pre- and Post-World War II Innovation Scores Compared

Prewar			Postwar			Change in Rank
	State	Score		State	Score	
1.	Minnesota	.784	1	New Jersey	.484	+33
2.	Texas	.699	2.	Louisiana	.428	+22
3.	Washington	.606	3.	Kentucky	.390	+ 1
4.	Kentucky	.561	4.	California	.389	+ 5
5.	North Carolina	.557	5.	Oklahoma	.367	+18
6.	Tennessee	.547	6.	Connecticut	.365	+11
7.	Missouri	.528	7.	Michigan	.358	+21
8.	Kansas	.524	8.	New Hampshire	.354	+37
9.	California	.473	9.	Minnesota	.352	– 8
10.	South Carolina	.449	10.	Illinois	.350	+11
11.	Montana	.444	11.	Pennsylvania	.331	+ 9
12.	Georgia	.437	12.	New York	.330	+18
13.	Ohio	.427	13.	Washington	.322	–10
14.	Wisconsin	.426	14.	Missouri	.312	– 7
15.5	Mississippi	.393	15.	Iowa	.308	+ 0.5
15.5	Iowa	.393	16.	Oregon	.300	+ 3
17.	Connecticut	.388	17.	Texas	.292	–15
18.	Nebraska	.376	18.	Ohio	.279	– 5
19.	Oregon	.375	19.	Wisconsin	.270	– 5
20.	Pennsylvania	.360	20.5	Colorado	.264	+ 6.5
21.	Illinois	.359	20.5	Arkansas	.264	+19.5
22.	Arizona	.351	22.	Indiana	.252	+ 3
23.	Oklahoma	.350	23.	Georgia	.249	–11
24.	Louisiana	.340	24.	Florida	.239	+ 7
25.	Indiana	.335	25.	Mississippi	.224	– 9.5
26.	West Virginia	.321	26.5	South Dakota	.221	+ 6.5
27.	Colorado	.314	26.5	Delaware	.221	+16.5
28.	Michigan	.310	28.	Tennessee	.219	–22
29.	Alabama	.275	29.	South Carolina	.216	–19
30.	New York	.272	30.	Alabama	.206	– 1
31.	Florida	.268	31.	Maryland	.194	+ 5.5
32.	Rhode Island	.262	32.	North Dakota	.175	+ 6
33.	South Dakota	.241	33.	North Carolina	.172	–28
34.	New Jersey	.218	34.	Utah	.169	+ 7
35.	Idaho	.216	35.	Nevada	.157	+13
36.5	Maryland	.214	36.	Arizona	.154	–14
36.5	Virginia	.214	37.	Idaho	.146	– 2
38.	North Dakota	.210	38.	Kansas	.138	–30
39.	Massachusetts	.189	39.	Nebraska	.124	–21
40.	Arkansas	.178	40.	Alaska	.116	+ 9
41.	Utah	.174	41.	Hawaii	.096	+ 9
42.	Maine	.173	42.	New Mexico	.095	+ 2
43.	Delaware	.163	43.	Montana	.083	–32
44.	New Mexico	.127	44.	Rhode Island	.079	–12
45.	New Hampshire	.123	45.	Vermont	.075	+ 1
46.	Vermont	.072	46.	West Virginia	.055	–20
47.	Wyoming	.041	47.	Virginia	.045	–10.5
48.	Nevada	.030	48.	Massachusetts	.036	– 9
49.	Alaska	.013	49.	Wyoming	.004	– 2
50.	Hawaii	.000	50.	Maine	.000	– 8

Source: Collected and compiled by the authors.

them (r = .450). But the table shows that some dramatic changes took place between the two periods. New Hampshire and New Jersey climbed over 30 places, Louisiana and Michigan over 20 places. Meanwhile, Kansas, Montana, Nebraska, North Carolina, and Tennessee all dropped more than 20 places. Twenty-four states shifted at least 10 ranks in one direction or the other, and only three states were in the top 10 in both periods. The mean shift in rank for a state was 12.0.

Table 3 summarizes the states' rankings by census region. In the prewar period, the most innovative appellate courts came largely from relatively rural states of the South and the great plains farm belt. By contrast, most of the heavily industrialized states of the Northeast and North Central regions were in the lower half of the rankings. In the postwar period the patterns shifted somewhat. Many of the southern and plains states lost their penchant to adopt innovative tort doctrines early, and the more industrialized states of the Middle Atlantic and East North Central regions became much more innovative as a group. The regional patterns suggest some correlates of innovativeness related to state characteristics, and these will be examined later in this section.

Comparison with Legislative Innovativeness. We may compare the innovativeness of state legislatures and courts by comparing our innovation scores with the most comprehensive measure of legislative innovativeness, the scores obtained by Walker (1969, p. 883). This single comparison will not provide a definitive test of the thesis that innovativeness is a general characteristic of state governments, but it will shed some light on that thesis.

Overall, the two indices are moderately related, with a correlation coefficient of .276. Because of the changes that we found in judicial innovativeness over time, we also analyzed the prewar and postwar periods separately. We divided Walker's innovations into those with a median adoption by 1943 and those with a later median adoption, and we calculated separate innovation scores for the prewar and postwar legislation. We then found correlations between legislative and judicial innovativeness of .144 for the prewar period and .339 for the postwar period. The latter is a respectable relationship but still explains only 12 percent of the variance; the former explains virtually nothing.

To the extent that this comparison taps the relationship between legislative and judicial innovativeness, our findings suggest that innovativeness is not a basic characteristic of state governments as a whole. In the postwar era, however, it appears that some common forces helped to determine propensities toward innovation in two branches of state government. Our analysis thus far does not allow us to identify those forces.

It may be that receptivity to innovation as a

Table 3. Average Tort Law Innovation Scores and Rankings by Census Region

	Whole Period		Prewar Period		Postwar Period	
	Average	Rank	Average	Rank	Average	Rank
Northeast						
New England	.171	8	.201	9	.152	8
Middle Atlantic	.342	2	.283	7	.382	1
North Central						
East North Central	.330	4	.371	4	.302	3
West North Central	.313	5	.437	2	.233	6
South						
South Atlantic	.234	7	.328	5	.174	7
East South Central	.335	3	.444	1	.260	4
West South Central	.359	1	.392	3	.338	2
West						
Mountain	.163	9	.212	8	.134	9
Pacific	.263	6	.293	6	.245	5

Source: Collected and compiled by the authors.

Note: The composition of the regions is as follows. *New England:* Maine, N.H., Vt., Mass., R.I., Conn.; *Middle Atlantic:* N.Y., N.J., Pa.; *East North Central:* Ohio, Ind., Ill., Mich., Wisc.; *West North Central:* Minn., Iowa, Mo., N.D., S.D., Neb., Kansas; *South Atlantic:* Del., Md., Va., W.Va., N.C., S.C., Ga., Fla.; *East South Central:* Ky., Tenn., Alab., Miss.; *West South Central:* Ark., La., Okla., Texas.; *Mountain:* Mont., Idaho, Wyo., Colo., N.M., Ariz., Utah, Nevada; *Pacific:* Wash., Oregon, Calif., Alaska, Hawaii.

general characteristic of state governments is modified by a state's political ideology. That is, some states may be inclined to adopt politically liberal innovations such as those that favor consumers and workers while others are quick to adopt conservative innovations such as those favoring business and industry. If this is true, comparison of our judicial innovation scores for liberal, consumer-oriented tort doctrines with legislative innovation scores derived from a very broad range of issues might be misleading. Accordingly, we calculated innovation scores for two subsets of Walker's 88 legislative innovations: 31 laws which can be characterized as liberal; and 12 of those laws which potentially redistribute resources from relatively wealthy institutions to consumers of their products and services. Each of these subsets also was divided into prewar and postwar groups.

Somewhat surprisingly, controlling for ideology and subject matter does not enhance the relationship between legislative and judicial innovativeness. The correlation coefficients for the whole period, and for the prewar period, are well below those obtained with undifferentiated sets of innovations, while the coefficients for the postwar period are at about the same level (.338 for the liberal innovations; .286 for the redistributive). In short, if there is a characteristic of state government receptivity to innovation, it does not seem to be a function of ideological direction or policy area.

Characteristics of States. Although legislative and judicial innovativeness may be related only modestly, both might stem in part from state characteristics. Walker (1969, pp. 883-84) found strong statistical relationships between his innovation scores and population, urbanism, wealth, and industrialization. Other studies of innovation also have found characteristics of the environment to be important (Feller and Menzel, 1977; Aiken and Alford, 1970, pp. 851-61). Such findings suggest the importance of relevant state attributes at least as correlates of judicial innovativeness and perhaps as explanatory factors.

The most important demographic characteristic probably is *population*. The level of a state's population will be reflected in the volume of litigation. This is significant because innovation is dependent upon the existence of appropriate cases. A sparsely populated state such as Wyoming simply may generate too few cases to allow its courts to adopt doctrines before most other states. In contrast, courts in a heavily populated state such as California may receive rapid demands for the adoption of innovative doctrines.

Urbanization and *industrialization* may affect innovation in several ways. They increase interactions of the sort that lead to litigation. Urban-industrial areas are likely to be relatively sophisticated and cosmopolitan, and these characteristics in individuals render them more receptive to innovations (Rogers and Shoemaker, 1971, p. 186 ff.). Finally, attorneys in urban-industrial states may well be better trained and more aware of developments in the law than their rural counterparts.

We tested the relationships between our innovation scores and these three characteristics for the two periods. Prewar scores were analyzed in relation to Hofferbert's demographic data for 1920, and postwar scores with his 1960 data (1968). As Table 4 shows, only population was positively related to innovation during both periods. That relationship was moderately high and statistically significant in both periods, and it remained significant when controls for the other demographic variables were introduced. In the postwar period all three demographic variables had highly significant positive relationships with innovation; the introduction of controls establishes that population was the most important of the variables and that urbanization had little independent effect. The change in the relationship between industrialization and innovation over time suggests that the level of industry in itself has not been important for innovation. Urbanization appears to be uniformly unimportant. Certainly the most important implication of these findings is support for our speculation that sparse population reduces opportunities for judicial innovation.

These results suggest a basis for the relationship between legislative and judicial innovation. The moderately strong relationships between our innovation scores and those obtained by Walker in the postwar period may well be a product of the influence of demographic variables—particularly population—on both legislative and judicial action. Indeed, controls for the demographic variables eliminate the statistical relationship between the legislative and judicial innovation scores in the postwar period. Perhaps innovativeness as a general characteristic of states, to the degree that it exists, is a result of demography rather than attributes of the political system.

Innovativeness in tort law also might be related to two political characteristics of states: prevailing ideology and political culture. The regional patterns seen in Table 3 make political ideology particularly promising. In the prewar period the Populist and Progressive movements might have encouraged the adoption of legal doctrines favoring individuals over corporate interests. In the postwar period New Deal liberalism might be associated with the adoption of liberal tort rules.

To test the impact of political ideology we analyzed the relationship between innovation scores

Table 4. Relationships between Tort Law Innovation Scores and Demographic Variables (Correlation Coefficients)

Innovation Scores	Population	Manufacturing	Urbanization	R	R²
Prewar					
Zero-Order	.297*	−.134	−.036	–	–
Controlling for Other Demographic Variables	.366**	−.261	.139		
Postwar					
Zero-Order	.470**	.409**	.333**	.512	.262
Controlling for Other Demographic Variables	.303*	.198	.041		

Sources: Demographic data from Richard I. Hofferbert, "Socioeconomic, Political and Public Policy Data 1890-1960," dataset obtained through Inter-University Consortium for Political and Social Research; innovation data collected and compiled by the authors.

*Significant at .05 level.
**Significant at .01 level.

and voting patterns. In the prewar period we used the proportions of votes obtained by three Populist and Progressive candidates for president (Bryan in 1896, Roosevelt in 1912, and La Follette in 1924) and the success of Progressive party candidates in state elections in the 1910-1912 period (see Gray, 1973, p. 1183). In the postwar period we used the proportions of votes obtained by Democratic candidates in 1936 and 1964 and the combined support for Truman and Wallace in 1948. We included the 1936 election, even though it occurred in the prewar period, because the divisions in that election reflected ideological coalitions that were typical of the postwar period.

The results suggested a lack of impact for ideology. While the relationships between party voting and innovation generally were in the posited direction, in no case were they statistically significant. Controls for the impact of demographic variables and exclusion of southern states in the postwar period did not change this pattern. It may be that dominant state ideologies have had an effect on some state court systems. But state political ideology—at least as measured by general election results—was not reflected systematically in the innovativeness of state courts in tort law.

The importance of political culture for innovation is suggested by Elazar's pioneering analysis of state political cultures (1972, pp. 149-50). He argues that what he calls the "moralistic" states should be the most innovative, the "traditionalistic" states least innovative, with the "individualistic" states in between. Johnson (1976, pp. 502-06) showed that this argument held true for Gray's index of legislative innovation, but not for Walker's index. We tested the relationship between tort innovation and political culture; our measure of culture was Sharkansky's quantification of Elazar's state cultures, which ranks states from moralistic to traditionalistic (1969). The results were entirely nonsignificant. No meaningful relationship appeared between culture and innovation for the whole period or for the two subperiods, with or without controls for population.

Characteristics of Courts. Attributes of courts and court systems might have a stronger effect on innovation than attributes of states because their effect is more direct. One cluster of court attributes which may be significant is judicial professionalism. Scholars and practitioners concerned with judicial administration have argued that certain kinds of "professional" features enhance the capacity of courts to function effectively (Winters and Allard, 1965; Rubin, 1976, pp. 208-12). Among the features referred to as professional are the presence of professional administrators, the existence of an intermediate appellate court, and "merit selection" of judges. In the aggregate, these features may create conditions that are favorable for innovation. Professional administrators and intermediate appellate courts may give supreme court justices more time to become aware of innovative doctrines and to consider their implications. Merit selection procedures may produce judges who are more sensitive to needs for doctrinal change.

The best available data on judicial professionalism are the composite scores for state court systems created by Glick and Vines (1973, p. 12). These scores reflect five factors: method of judicial selection, state court organization, judicial administration, judges' tenure in office, and the level of basic judicial salaries. Because the scores are based on postwar conditions, we analyzed them in relation to our postwar innovation scores only.

The relationship between innovation and professionalism is moderate (r = .274, significant at the .05 level). But judicial professionalism is strongly related statistically to population, industrialization, and urbanization,[4] and controlling for any of them reduced the relationship to virtually zero. Thus professionalism seems not to have an independent effect on innovation.

A second type of court characteristic that may be related to a court's readiness to adopt innovations is the backgrounds of its judges. Because of the weakness of political pressures for change in common law fields and the absence of major resource needs for innovation, judges' decisions whether to adopt innovations may be based chiefly on their personal preferences. Those preferences in turn may reflect and be reflected in their background characteristics.

Three background characteristics seem most likely to be linked with inclination to innovate in tort law. The first is *party affiliation.* Democratic judges as a group take more liberal positions than do Republicans, and one component of this liberalism is a sympathy for plaintiffs in tort cases (Nagel, 1961, pp. 844-45; Ulmer, 1962, pp. 355-62). A second is judges' *legal education.* Judges with law degrees from prestigious, top-quality law schools are likely to have greater knowledge of legal developments and more confidence about adopting innovations than judges from other institutions. The third is the *locale* of judges' last private *law practice.* Judges who practiced in metropolitan areas may be more likely than other judges to have entered communication networks that make them aware of innovations in other states, and they may be less resistant to legal change. Notably, Jaros and Canon (1971) found that on state supreme courts both justices from metropolitan practices and those with prestigious law degrees had relatively high dissent rates, and there may be a relationship between willingness to cast dissenting votes and innovativeness.

We analyzed the impact of these three characteristics in the postwar period with the data collected by Canon and Jaros (1970) on the backgrounds of state supreme court justices who served between 1960 and 1968. These data are not ideal, because they cover only a small part of the postwar period. But the long service of many justices and the stability that probably exists in background characteristics in many states made it reasonable to use these data at least for exploratory purposes.

We found little relationship between the states' innovation scores and the number of justices with prestigious law degrees (r = .192), and the relationship with the number of Democrats was even weaker (r = .066; .064, with the South excluded). But the correlation between number of justices with metropolitan practices and innovation scores was a dramatically significant .543. Even with controls for all three demographic variables, the partial correlation for metropolitan practice and innovation remains moderately high at .318, considerably higher than for any of the demographic variables and innovation with an equivalent set of controls.

The absence of a relationship between justices' party affiliations and judicial innovativeness is consistent with the unimportance of party voting patterns as an explanation for states' receptivity to judicial innovations. The complexity of the relationship between party and ideology at the individual and state levels undoubtedly contributed to these negative findings. It also may be that ideological liberalism is less closely related to support for innovations that favor tort plaintiffs than we had posited.

Because of the limitations of our data, the relationship between metropolitan law practice and innovation should not be emphasized. The relationship may be limited to a particular period, and it might be largely spurious because of the impact of variables that we did not analyze. But we think that it makes intuitive sense for the reasons that we suggested. Urban judges, like urban residents generally, are most likely to have the information and orientations that support innovation. Moreover, the urban or rural background of the bench should have a much more direct effect on judicial behavior than the urbanization of the state as a whole.

Innovativeness as a Consistent Judicial Trait. We have shown considerable variation among state court systems in tort law innovation and a relationship between this variation and some characteristics of states and courts. But we have not yet explored the consistency of judicial innovativeness. To what extent is the order of adoptions similar across our tort doctrines? Our comparison of prewar and postwar innovation scores indicates that there is no strong consistency over time in the innovativeness of state court systems. But that finding does not indicate how similar the patterns of innovation are within shorter periods.

We examined the consistency of innovativeness through analysis of intercorrelations between the sets of adoption scores for pairs of doctrines and through factor analysis of adoption scores. The intercorrelations between doctrines depict a modest level of consistency in the prewar era. The median was .210, but only 28 percent were greater than .300. No consistency occurred in the postwar doctrines. The median was .079, and more intercorrelations were negative (25 percent) than were above .200 (23 percent). The median intercorrelation for all doctrines in both periods was .113.

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[4] The correlations between demographic variables and professionalism have coefficients ranging from .515 to .652. The multiple R for all three demographic variables is .704.

Nor did we find strong relationships between adoption patterns for doctrines that are particularly similar in subject matter (see Eyestone, 1977, pp. 443-44). For instance, five doctrines were involved in the movement toward strict liability for the manufacture and sale of defective products. The intercorrelations among these doctrines range from .020 to .350, with a median of only .199. The only exception to the pattern of weak relationships between similar doctrines is a correlation of .467 between the abrogation of charitable immunity and the abrogation of municipal immunity. The low correlations between related doctrines might be explained in a few instances by overlap between them that allows a court to choose one to the exclusion of another, but for the most part that degree of overlap did not exist.

The intercorrelations suggest an absence of consistency in the pattern of adoptions of innovative doctrines. Factor analysis provides a useful second test of consistency because it is oriented toward finding structure in the data. We undertook three forms of factor analysis with the doctrines as variables and the states as cases. Each involved principal factoring with iteration; the alternative methods were oblique rotation and varimax and equimax criteria for orthogonal rotation.

The analyses yielded similar results that corroborated the findings of the intercorrelation analysis. Most generally, the analyses showed that the adoption patterns had a highly complex structure. First of all, a great many factors were required to describe these patterns. Prior to rotation, using the criterion of eigenvalues greater than 1.0, ten factors were needed to account for the structure of the data. Moreover, as the large number of factors suggests, only a few variables loaded at high values on any single factor. In the varimax analysis, for instance, no more than four doctrines had loadings of .400 or above on any single factor.

These findings argue against the idea that doctrinal innovativeness is a stable trait of state court systems. The inconsistent pattern within a single issue area may stem chiefly from the courts' dependence on litigants to initiate the innovation process in a particular state. But it does provide additional evidence against the proposition that institutions have strong general propensities toward innovation.

The difference in the median intercorrelations between doctrines in the prewar and postwar periods is of some interest. It seems likely that the lower intercorrelations in the postwar period are related to the relative speed of adoptions in that period (which we will discuss in the next section). Where a new doctrine sweeps through the courts, the order in which states accept it almost surely depends less on any systematic factors than on the speed with which appropriate cases reach particular courts.

The general inconsistency of innovation patterns has implications for the determinants of innovativeness. This inconsistency makes it less likely that certain characteristics of states or of courts have a uniform impact on judicial innovation. Thus it helps to explain the weak relationships that we found between innovativeness and some characteristics that would seem to be related to innovation.

Data and Analysis: The Pattern of Diffusion

Regionalism in the Diffusion Process. Some studies of legislative innovations have shown a regional element in the diffusion pattern (Walker, 1969, pp. 891-96; Foster, 1978). This regionalism has been explained by the existence of regional communication patterns and by a tendency for officials and citizens to measure their state's performance and programs against those of neighboring states.

The strength of these forces for regionalism in the courts is difficult to determine. State judicial decisions are recorded in regional reporters, but these reporters may be obtained fairly readily on a national basis. Lawyers and judges are trained to view the legal system as national, but they still may look primarily to neighboring states for cues and yardsticks. The ambiguity of this picture is reflected in the findings of the major study of citation patterns in state supreme court opinions: judges disproportionately cite the opinions of courts in nearby states, but this tendency is not overwhelming (Harris, 1979, pp. 19-32).

We cannot investigate the role of regionalism in the process by which state judges learn of innovations and choose to adopt them. But we can investigate regionalism in the patterns by which innovations spread. To do so we have adopted Walker's methodology: a varimax factor analysis with the states as variables and doctrines as cases. As in Walker's analysis, consistent patterns of diffusion within a region will be reflected in a clustering of the states in that region on a single factor.[5]

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[5] One alternative procedure would have been to perform separate factor analyses by state on the sets of doctrines that cluster together. The lack of structure in the analysis of doctrines, such that few doctrines clustered together, made this procedure impossible.

The results of the analysis for the entire period are shown in Table 5, which lists the states with loadings of at least .400 on the factors that explain the most variance. The data in that table offer a mixed picture. There is some evidence of regionalism in the first two factors, both of which have a majority of southern and border states among the highest loaders. But factor 1 is perhaps best seen as a clustering of nonindustrial states. None of the remaining factors has a strong regional cast. Factor 6 is of interest because it includes four of the five leading innovators, all except Kentucky.

We also undertook separate factor analyses for the prewar and postwar periods. The patterns that emerged must be interpreted with great caution because of the small numbers of doctrines that we analyzed in each period. But generally we found that the effect of regionalism was even weaker in these subperiods than it was in the entire study period.

Certainly our findings indicate that regionalism plays a far weaker role in defining factors for judicial tort innovations than was the case for Walker's legislative innovations. To the extent that we can identify regional elements in the factors, they are not very strong. Our findings suggest that state appellate courts are open to influence from other courts regardless of location.

More generally, the factor analysis discloses a weak structure to the tort diffusion pattern. Walker found that all but three states loaded at the .400 level or higher on one of the five major factors that he identified; fewer than two-thirds of the states met that criterion for the first six factors in our analysis of the whole period. This finding supports the conclusion that the pattern of adoption for tort doctrines is largely idiosyncratic.

The Speed of Diffusion. It is difficult to say whether innovative policies generally diffuse more rapidly among courts than among legislatures. Diffusion among legislatures is slowed by several factors that should be less important in the courts, including communication weaknesses, the existence of multiple decision points, and the need to weigh political pressures. But this advantage may be offset by courts' dependence on litigants for action. Moreover, judicial acceptance of innovations frequently is slowed by adherence to precedent and deference to the legislature as the appropriate source of major legal change.

Once again we may use Walker's study for comparison. Walker (1969, p. 895) found that the first 20 adoptions of legislation required an average of around 20 years, declining slightly from 23 years in the late nineteenth century to 18 years in the mid-twentieth century. The rate of diffusion for our tort innovations was markedly slower. For the entire set of doctrines (excluding one that was adopted by fewer than 20 states), the average time required for 20 adoptions was 37.1 years. For the 12 doctrines first adopted prior to 1920, the mean period was 46.5 years; for the 10 adopted later, the mean declined to 25.9 years. Even that figure is 8 years greater than the rate for legislation in the most comparable period.

Because these findings are based on a single limited comparison, they do not allow firm conclusions. But they do suggest the importance of judicial passivity and restraint in delaying the adoption of innovations.

Table 5. Varimax Rotated Factor Analysis of State Innovation Scores (States with Loadings of at Least .400 on First Six Factors)

Factor 1 (15.8% of variance)	
Montana	.862
Nebraska	.821
Georgia	.799
Missouri	.712
Tennessee	.686
West Virginia	.658
Arizona	.548
Virginia	.531
Kentucky	.424
Iowa	.406
Factor 2 (10.7%)	
Alabama	.945
South Carolina	.845
Oklahoma	.528
Idaho	.497
Louisiana	.467
North Carolina	.444
Ohio	.427
Factor 3 (9.4%)	
New Jersey	.904
Hawaii	.693
Oregon	.502
Factor 4 (8.4%)	
Colorado	.910
Virginia	.521
Connecticut	.469
North Dakota	.468
Factor 5 (7.9%)	
Maine	.978
Massachusetts	.956
Indiana	.485
Virginia	.435
Factor 6 (7.4%)	
Washington	.820
Texas	.680
Minnesota	.632
Rhode Island	.577
California	.516

Source: Collected and compiled by the authors.

The growing speed of diffusion for the tort law that we found in the past half century is in part an artifact of our selection of doctrines. A doctrine that was first adopted in 1950 and that has diffused fairly slowly would not have met our criterion for inclusion. But there have been real changes as well. Of the six doctrines that were adopted by 20 states in fewer than 20 years, for instance, five were first adopted in 1950 or later. It is much more common for large numbers of courts to adopt a doctrine quickly than it used to be.

This change can be traced to both litigants and courts. The growing volume of litigation has reduced the time that an appellate court must wait for a chance to act. Moreover, professionalization and improved communication within the personal injury bar have given plaintiffs greater capacity to argue for the adoption of innovations.

Probably more important are changes in the courts. Judges have become more aware of doctrinal developments in other states' appellate courts and in law review commentaries; this growing awareness is reflected in an increasing self-consciousness with which innovations are discussed and adopted in judicial opinions. In addition, the traditional nineteenth-century view that law was a permanent entity has gradually eroded. Many modern judges have adopted the view that the law should be flexible and responsive to social needs, and in doing so they have developed a more favorable attitude toward doctrinal innovation.

Conclusion

The most important implication of our findings for the study of state judicial systems concerns the dramatic shift in the geographical locus of doctrinal innovativeness over time. Our analysis of this change in conjunction with Populist, Progressive and New Deal voting patterns revealed little ideological linkage. Unfortunately, we have few insights into other causes of this shift because scholars who have studied state court policy making longitudinally have largely passed over the first half of the twentieth century. Major legal historians such as Hurst (1950) and Friedman (1973) have focused on the nineteenth century while others have concentrated upon post-World War II legal developments (e.g., Keeton, 1969). Nonetheless, the geographical and social nature of the shift in judicial innovativeness in tort law is both striking and unconventional and calls for further study to determine what has caused it and whether it is part of a more general phenomenon.

Our findings have two noteworthy implications for diffusion theory. First, the reactive position of the courts is an important explanatory consideration. Because courts are dependent upon litigants' demands, a strong element of idiosyncracy governs the diffusion of tort doctrines. Moreover, it is doubtful that this idiosyncracy is peculiar to one branch of the law.

The courts' reactive role contrasts with the initiatory powers of legislatures and administrative agencies. While policy making in these institutions is often responsive to external events, they are usually aggregate events. The courts' lack of control over the timing of the adoption of innovations is reflected in the considerable difference between the relatively systematic diffusion patterns that Walker (1969) and others have found outside the judiciary and the seemingly unstructured patterns we have found.

This difference is important to diffusion theory because courts are by no means the only reactive policy makers in our political and social systems. Other institutions must on occasion adopt policies in response to relatively random events. For instance, state and local governments must respond to natural disasters or urban disorders, business firms to new competitive products or attempts at corporate takeovers. Our findings suggest that conventional explanations for the diffusion of innovations have limited application in such situations.

The second implication stems from our finding little evidence of regionalism in the diffusion of tort doctrines. This is in considerable contrast with Walker's data about diffusion in the legislative arena. While geographical proximity is undoubtedly an important aspect of diffusion, we suspect that a process which transcends regionalism is the wave of the future. The legal system developed rather early a method for communicating court decisions that is both formally structured and geographically unlimited. Only in the current era are many other political and social systems developing social and technological avenues of information exchange which override geographical barriers. This is perhaps reflected in Walker's having noted a diminution of regionalism in the diffusion of new laws since the 1930s (1969, p. 896). We would expect this trend to continue so that the pattern of diffusion that we found in the judiciary will become common in the legislature as well as in other institutions.

Appendix. Twenty-Three Plaintiff-Oriented Tort Doctrines (with Year of First Adoption)

1. Attractive nuisance doctrine: 1858.
2. Single indivisible injury rule: 1860. (Plaintiffs do not have to show apportionment of exact damages to each joint defendant.)
3. Negligence per se doctrine: 1889. (Violation

of safety law or ordinance by defendant is considered negligence in itself.)

4. Covenant to repair doctrine: 1890. (Failure to make promised repairs in premises makes landlord liable for damages therefrom to tenants or guests.)
5. Abrogation of the impact doctrine: 1890. (This doctrine required a physical impact on the plaintiff before recovery for injuries was allowed.)
6. Right to privacy: 1905.
7. Assured clear distance doctrine: 1909. (Burden on defendant to show reasonable distance between vehicles before rear-end collision.)
8. Family purpose doctrine: 1911. (Allows recovery from head of household for vehicular negligence by any member of household.)
9. MacPherson doctrine: 1911. (Overcomes privity of contract defense for injury due to negligent manufacture of product potentially harmful to life or limb in ordinary use.)
10. Strict liability for defective food or drink: 1913.
11. Abrogation of interspousal immunity: 1914.
12. Medical malpractice discovery rule: 1917. (Statute of limitations runs from discovery of injury, not from time medical service performed.)
13. Abrogation of charitable immunity doctrine: 1920. (This doctrine rendered charitable institutions such as hospitals immune from liability for torts.)
14. Connecticut rule: 1921. (In multi-unit dwellings, landlord is liable for injuries resulting from failure to remove ice and snow from walkways.)
15. Permitting recovery for wrongful prenatal death: 1923.
16. Baxter doctrine: 1932. (Treats advertisements concerning product safety as equivalent to explicit warranty.)
17. Abrogation of municipal immunity: 1945.
18. Abrogation of accepted work doctrine: 1949. (Under this doctrine contractor's tort liability for negligent construction terminated with acceptance of the work by contracting party.)
19. Per diem formula: 1950. (Permits plaintiff's attorney to suggest to jury the award of a daily rate for pain and suffering.)
20. Expansion of consortium: 1953. (Allows wives as well as husbands to recover for loss of consortium.)
21. Builder-vendor implied warranty doctrine: 1957. (Builder-vendor of dwelling places liable in tort for negligent construction despite transfer of title.)
22. Abrogation of privity in warranty: 1958. (Under this doctrine warranty extended only to purchasers of product, not other users.)
23. Strict liability for harm-causing defects in manufactured products: 1963.

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Toward a Diachronic Analysis of Congress

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This article assesses the status of diachronic research on the United States Congress. A literature review reveals a lack of truly diachronic studies, but a wealth of insightful and useful historical and contemporary studies. Drawing on organization theory, a research approach designed to facilitate and improve diachronic analysis is put forward. A major focus of the approach is on the ways in which environmental factors—both fixed and variable—shape the operations and performance of Congress. In addition, problems and strategies in analyzing the impacts of Congress on the wider political system are examined. The concluding section presents a research agenda for political scientists interested in applying diachronic analysis to the study of Congress.

Our aim in this article is to review research on the United States Congress which treats change over time as a major factor, to explain the basic premises and rationale of such an approach, and to indicate how analysis of this kind can best be pursued. We shall rely on the term "diachronic" rather than the term "developmental" because the latter has unavoidable judgmental implications. In contrast, the term "diachronic" is neutral. It simply means relating to or dealing with phenomena that occur or change over time.

Premises and Questions

Most studies of Congress have analyzed questions of structure, behavior, or performance at fixed or limited points in time. On occasion the time-bound character of the research has been recognized explicitly. Often, however, it has not and authors have implied or assumed that their findings have general validity. Yet each of these aspects of congressional functioning is subject to substantial change. The House of the 1980s, for example, differs markedly from the House of the 1810s, the 1890s, or even the 1940s in seniority norms, partisan behavior, and output activities. Similarly, Senate norms, practices, and output activities have changed substantially over time. This is not to argue that some constants do not also exist. Our point is that static approaches have difficulty both in identifying key independent variables and in treating the ranges and effects of the independent variables they do identify.

Diachronic Analysis. Increasingly since the late 1960s, students of Congress and of legislatures more generally have begun to acknowledge that time-free research can easily result in time-bound, not timeless, findings. Malcolm Jewell and Gerhard Loewenberg commented recently that "our aspirations . . . must be to formulate generalizations broad enough to stand the test of time" (1980, p. 330). In congressional research, attaining such generalizations hinges on the ability of researchers to analyze Congresses of different time periods and the transitions between them in a coherent and conclusive fashion. In short, the explanation of current aspects of structure, behavior, and performance remains the critical research objective, but the concepts and measures required to provide an adequate basis for such explanation need to be developed through diachronic, rather than merely static, analysis.

Diachronic analysis of the U.S. Congress assumes and requires reliance on historical data and methods. However, it is not history as a set of events that is of primary interest, but rather history as a laboratory for testing theories. Moreover, history itself cannot be the direct source of the theories or measures applied. Rather, the concepts involved in building diachronic theories must derive from intellectual frameworks that are independent of history. Similarly, the measures applied to test such theories will require the exercise of empirical ingenuity in operationalizing key concepts.

Two further aspects of a diachronic approach to congressional analysis should be noted. First, though diachronic analysis lacks the range of comparative legislative analysis (Loewenberg and Patterson, 1979), it also has advantages and it also is comparative. The approach focuses on explaining congresses in different time periods and it can do so without confronting the complexities posed by highly variegated cultures, societies, and political systems. It thus provides a basis for understanding congressional change and for building more general theories of legislative change. Second, since diachronic analysis involves explaining change over time, it directs attention to the context as well as the unit, to exogenous as well as endogenous factors and the interaction between the two. The approach is accordingly systemic in its concerns and its orientations are of the open system variety. The underlying model is biological rather than mechanical. Change is assumed to be additive (though not necessarily positive or functional) rather than revolving around some inherent or ideal point of equilibrium. This basic premise underlies the salient differences separating static from diachronic approaches. In fact, both are concerned with change or variation, and both need evidence of change or variation to develop and test explanatory theories. However, static analysis assumes change or movement within a closed system, where conditions of equilibrium prevail, and thus encourages the development of theories grounded in cross-sectional analysis. In contrast, diachronic analysis assumes that change is ever unfolding and thus is primarily concerned with the interaction between variables over extended periods of time.

Organizational Framework. In this article we shall rely on the distinction between independent and dependent variables and the concepts of operations and performance. The identification of independent and dependent variables is typically arbitrary in the sense of being relative to the assumptions and aims of inquiry. Nonetheless, congressional scholars often approach their research in cause and effect terms and this distinction therefore provides a convenient way of broadly organizing major portions of our analysis. The concepts of operations and performance will be subject to recurrent explanation and illustration in the course of this article. At this point they may simply be identified as the basic points of reference for analysis of Congress as either an independent or dependent variable. In research which treats Congress as a *dependent* variable, the primary questions relate to explaining change in aspects of operations and performance. Inquiry thus focuses on explaining change in congressional structure, behavior, and outputs. In research which treats Congress as an *independent* variable, the primary questions relate to explaining the impacts of congressional operations and performance on the wider political system. Inquiry thus focuses on explaining the differences Congress makes for the functioning of the system as a whole.

Congress as a Dependent Variable: The State of Knowledge

To review research that bears on the explanation of change in congressional operations and performance, one must cast a wide net. This is true both because diachronic analysis is new and underdeveloped and because more familiar types of congressional research can have relevance for diachronic analysis, even when guided by other perspectives and concerns.

Historical Studies. Historical research on congressional operations and performance can be classified in three categories. These categories, together with examples of studies that fit them, are listed in Table 1. A small and relatively recent portion of the historical literature can be viewed as diachronic since it applies a related set of concepts and measures to the analysis of change over an extended period. The prime example is Polsby's study (1968) of the institutionalization of the House of Representatives. In addition, a few specific aspects of congressional operations, such as seniority and party voting, have been studied diachronically. All these studies are instructive and worthwhile. However, they represent only initial or tentative attempts at diachronic analysis and remain limited in their explanatory power or range. We shall support this claim later in this article by analyzing Polsby's work on institutionalization in detail.

A second segment of the historical literature consists of general treatments of congressional operations and performance. A number of scholars have written broad histories of the House or Senate or traced the development of important aspects of structure, behavior, and performance over extended periods. These works have relevance for diachronic analysis as an information source. However, they were not designed to provide precise causal analysis and their general and descriptive character restricts their overall utility. In addition, a few scholars have examined broad patterns of institutional change in terms of environmental determinants or organizational needs. This work is analytic and suggestive, but its lack of operationalization limits its conclusiveness and applicability.

By far the largest portion of historical research on congressional operations and performance is periodic. The Federalist and Jeffersonian eras have been the subject of a variety of studies. Congresses in the middle decades of the nineteenth century have also been examined in some detail. Late nineteenth- and early twentieth-century congresses have been examined most intensively of all. Finally, various aspects of congressional operations and performance between the two world wars have been the topics of a number of studies.

The periodic historical literature provides a very useful source of evidence on change in various aspects of congressional operations and performance. In general, however, these studies have a number of limitations when viewed from a diachronic perspective. Though usually detailed and informative, most are essentially descriptive, rather than analytical. More important, even the studies that employ contemporary social science concepts and measures typically apply them over limited time frames to particular topics and do so in an isolated manner. Thus, even the most sophisticated of these studies tend to stand more as cross-sectional analyses, historically defined, than as diachronic analyses. As such, and the dis-

Table 1. Congress as a Dependent Variable: Historical Studies

Type of Study	Author*
I. Diachronic	
A. Institutionalization of the House	Polsby (1968)
B. Tenure and Turnover	Witmer (1964); Price (1971, 1975); Fiorina et al. (1975)
C. Seniority	Abram and Cooper (1968); Polsby et al. (1969); Price (1977)
D. Party Voting	Clubb and Traugott (1977); Sinclair (1977a, 1981 in D. and O.); Turner and Schneier (1970); Brady et al. (1979)
E. Leadership	Ripley (1969a); Jones (1970); Nelson (1977)
II. General Historical	
A. House	Galloway (1961); MacNeil (1963); Bolling (1968)
B. Senate	Haynes (1938); Ripley (1969b)
C. Practice and Procedure	Alexander (1916); Luce (1922); Burdette (1940)
D. Committee Development	McConachie (1898)
E. Speakers' Role/Power	Follett (1896); Brown (1922)
F. Patterns of Change	Huntington (1965); Cooper (1970); Dodd (1981 in D. and O.)
III. Particular Periods	
A. Early Congresses	
1. Party Politics	Dauer (1953); Cunningham (1963); Young (1966); Bell (1973); Hoadley (1980)
2. Committee Development	Harlow (1917); Cunningham (1978b); Stidham (1895)
3. Presidential-Congressional Relations	White (1948, 1951)
B. Mid-19th Century	
1. Presidential-Congressional Relations	White (1954)
2. Voting	Alexander (1967); Silbey (1967); Russo (1972)
3. Party Politics	Bogue (1973); Benedict (1974, 1975)
C. Late 19th-Early 20th Century	
1. General	Wilson (1885); Rothman (1966)
2. Presidential-Congressional Relations	White (1958)
3. Speaker/Revolt Against	Atkinson (1911); Chiu (1928); Jones (1968)
4. Party: Politics and Voting	Holt (1967); Brady (1973); Brady and Althoff (1974)
5. Practice and Procedure	McCall (1911)
D. Between World Wars	
1. General	Luce (1926); Hasbrouck (1927); Herring (1940); Young (1942)
2. Coalitions and Voting	Holcombe (1940); Grassmuck (1951); Patterson (1967); Sinclair (1977b); Brady (1978)

Source: Compiled by the authors.

*D. and O.–Larry Dodd and Bruce Oppenheimer, eds., *Congress Reconsidered* (1977, 1981).

tinction is a relative rather than absolute one, their utility in providing a conceptual basis for distinguishing change from stability and explaining transitions from one era to another is impaired.

Contemporary Studies. Research on operations and performance in the contemporary Congress also has relevance for diachronic analysis. Table 2 provides a set of categories for reviewing such research from a diachronic perspective together with examples of work in each category.

Most research on Congress since 1946 has focused on the determinants of operations and performance without much, if any, regard for change. Nonetheless, the approaches and findings of students of the modern Congress are highly suggestive in a number of areas for those who wish to engage in diachronic research. The work that analyzes the role and significance of written rules or unwritten norms defines one such area. The numerous studies of decision making at individual and collective levels define another. The comparative analysis of committee operations and performance exists as a third area in which important variables have been identified and key relationships explored. A final area concerns analyses of policy making in terms of models of policy arenas, iron triangles, group politics, or specific output determinants.

In addition, research on the modern Congress has relevance for diachronic analysis for another reason. Congress in recent decades has undergone substantial change and researchers in the 1970s have begun to take increasing note of alterations in traditional patterns of structure, behavior, and output. In short, a portion of modern research provides direct analysis and evidence of change in

Table 2. Congress as a Dependent Variable: Contemporary Studies

Type of Study	Author*
I. Cross-Sectional	
A. Written Rules and Unwritten Norms	Huitt (1957, 1961); Matthews (1959); Fenno (1962); Froman (1967); Hinckley (1971); Asher (1973); Oleszek (1978); Baker (1980)
B. Individual and Collective Decision Making	Miller and Stokes (1963); Jackson (1974); Mayhew (1966, 1974a); Clausen (1973); Matthews and Stimson (1975); Hinckley (1972); Kingdon (1973); Asher and Weisberg (1978); Schneider (1979)
C. Committee Operations and Performance	Jones (1961); Fenno (1966, 1973); Manley (1970); Price (1972, 1981 in D. and O.); Ferejohn (1974); Shepsle (1978)
D. Policy Making	Truman (1951); Lowi (1969); Davidson (1977); Hurley et al. (1977); Ripley and Franklin (1980)
II. Emphasis on Change	
A. Incumbency	Erikson (1971); Bullock (1972); Mayhew (1974b); Cover (1977); Ferejohn (1977); Fiorina (1977a); Born (1979); Mann and Wolfinger (1980); Collie (1981)
B. Reform	Cooper (1975); Ornstein (1975); Davidson and Oleszek (1977); Jones (1977 in D. and O.); Davidson (1981 in D. and O.); Schick (1981 in M. and O.); Dodd and Oppenheimer (1981 in D. and O.)
C. Miscellaneous	
1. Norms	Ornstein et al. (1981 in D. and O.); Ornstein (1981 in M. and O.)
2. Leadership and Party	Peabody (1976); Sinclair (1978, 1981, and 1981 in M. and O.); Brady and Bullock (1980)
3. Staff	Fox and Hammond (1977); Malbin (1981 in M. and O.)
4. Roles or Functions	Fiorina (1977b); Aberbach (1979); Dodd and Schott (1979)

Source: Compiled by the authors.

*D. and O.–Larry Dodd and Bruce Oppenheimer, eds., *Congress Reconsidered* (1977, 1981).
M. and O.–Thomas Mann and Norman Ornstein, eds., *The New Congress* (1981).

various aspects of congressional operations and performance. Perhaps the best example is the large number of studies of incumbency that have emerged in response to Mayhew's seminal article on the vanishing marginals (1974b). The variety of studies that have examined the causes, politics, and results of reform efforts in the 1970s provide another prime example of an area of research in which students of Congress have directly confronted change. A final example concerns more general studies of change in important aspects of operations or performance, such as norms, leadership, staff growth, and roles or functions vis-à-vis the executive.

Yet, if research on operations and performance in the contemporary Congress has substantial relevance for diachronic analysis and provides a far richer body of analytical tools than the historical literature, it nonetheless shares some of the significant limitations of this literature.

Those portions of contemporary research which do not deal directly with change over time cannot discriminate among or order the impacts of dynamic and stable causes or determinants. Cross-sectional analysis is rather better suited to recognizing and encompassing variation in effects or dependent variables. The cost of this advantage, however, is that explanations of operations and performance decay in power over time. The ironic result is that much of modern research is likely, with the passage of time, to be transformed largely into more sophisticated additions to the periodic historical literature. Note, for example, the pre-1970s committee literature which is now dated given the events of the past decade.

Those portions of contemporary research which deal directly with change do not thereby escape similar types of limitations. In general, they tend as much to chronicle events and their immediate determinants and consequences as to explain change or place it in perspective. These efforts are thus hamstrung by being overly immediate in their time frames and overly concrete in their concepts and measures. Much of this research as well, with the passage of time, will be transformed largely into new additions to the periodic historical literature.

Congress as an Independent Variable: The State of Knowledge

Research that treats Congress as an independent variable can be divided into two categories: first, the consequences of congressional operations and performance for system maintenance; and second, a new area of research which also qualifies as an aspect of the study of Congress as an independent variable, the manipulation of the external environment for purposes of personal or partisan advantage. Table 3 is organized in terms of these categories and provides illustrative examples.

Institutional Impacts. The literature which focuses on Congress as a dependent variable is much greater in size than the literature which focuses on Congress as an independent variable. However, a number of the studies which seek to explain various aspects of congressional operations or performance also treat the broader impacts of their findings in a substantial fashion. Examples of such studies will therefore also be included in our review.

The historical literature includes a number of studies that merit notice. Huntington (1965) and Dodd (1981, in D. and O.) have analyzed congressional performance over extended periods. Both assume that the maintenance of Congress's role in the political system is dependent on its ability to respond effectively to major national problems and both find the modern Congress deficient in some critical regards. These two studies are perhaps the most diachronic in the existing literature, but both are thematic and do not combine theoretical analysis and measurement to the degree that Polsby's study of institutionalization does. In addition, some other portions of the general historical literature emphasize consequences. These include both studies that treat internal aspects of operations and performance over time and studies that trace the impacts of major party realignments on Congress.

The periodic historical literature also includes important work. Here too researchers at times have stressed consequences. This is especially true of research in political science before the behavioral revolution. From Wilson (1885) to Galloway (1946), research on Congress often involved assessments of performance as a major element of the analysis. In general, such research has been consistent in showing concern for the representativeness of congressional decision making, but has altered its perspectives considerably on Congress's position and institutional needs vis-à-vis the presidency. In addition, recent students of the Congress, who have focused on particular historical periods, have also paid substantial attention to questions of consequences. James Young's analysis (1966) of the manner in which congressional politics in the Jeffersonian era promoted systemic breakdown provides a prime illustration.

The historical literature that bears on Congress as an independent variable thus also contains a number of instructive and informative studies. Nonetheless, its utility for diachronic analysis is limited. The best of this work is very insightful, e.g., that of Dodd and Young. However, a large gap remains between the approaches and methods which characterize this literature and analysis which combines theory and data, distinguishes clearly and coherently between performance as a dependent and independent variable, and treats a broad range of systemic impacts over an extended time period.

Similar strengths and weaknesses characterize the literature that deals with the contemporary Congress' impact on the wider political system. Presidential power, bureaucratic expansion, increased reliance on the federal courts, and innovation in congressional structures and processes have stimulated a variety of studies that in one way or another address Congress' role and significance in the political system.

A number of authors have focused on the impacts of particular aspects of operations or performance. They have analyzed such topics as the dimensions of congressional initiative and influence in the lawmaking process, the effects of growing administrative power on the politics and impacts of congressional decision making, and the role and significance of oversight processes generally and the budgetary process in particular. Concomitantly, students of public policy have begun to direct their attention to the analysis of such questions as Congress' ability to do policy research, patterns of influence in the distribution of federal grants, and the consequences of vague statutory language.

In addition, a number of authors have sought to deal with the broad character of relations be-

Table 3. Congress as an Independent Variable: Literature Review

Type of Study	Author*
I. Institutional Impacts	
A. Historical	
1. General	Huntington (1965); Dodd (1981 in D. and O.); Brown (1922); Burns (1963); Sundquist (1973); Brady (1980)
2. Periodic	Wilson (1885); Galloway (1946); Young (1966); Rothman (1966)
B. Contemporary Studies	
1. Congressional Initiative	Johannes (1972); Orfield (1975)
2. Administrative Power	Dodd and Schott (1979)
3. Oversight/Budget Processes	Seidman (1975); Aberbach (1979); Ogul (1977 in D. and O.); Fiorina (1981 in D. and O.); Wildavsky (1979); Ellwood and Thurber (1981 in D. and O.); Schick (1981 in M. and O.)
4. Policy Influence	Lowi (1969); Schick (1976); Jones (1975, 1976); Rundquist (1978); Arnold (1979); Ray (1980)
5. Congressional Role and Status	Saloma (1969), Rieselbach (1977); Cooper (1975); Burnham (1959); DeGrazia (1965); Sundquist (1980)
6. Representation	Dexter (1962); Miller and Stokes (1963); Davidson (1969); Sullivan and O'Connor (1972); Weissberg (1978, 1979); Fenno (1978)
II. Environmental Manipulation	
A. Reelection Motives	
1. General Impacts	Mayhew (1974a); Fiorina (1977b)
2. Casework: Causes/Impacts	Johannes (1979, 1980); Arnold (1979); Ray (1980)
B. Other	
1. Personal and Institutional Conflicts	Cooper and Brady (1973); Davidson and Oleszek (1976); Dodd (1977 in D. and O.)
2. Committees and Member Goals	Fenno (1973)
3. Negative Institutional Standing	Fenno (1975); Parker and Davidson (1979)

Source: Compiled by the authors.

*D. and O.–Larry Dodd and Bruce Oppenheimer, eds., *Congress Reconsidered* (1977, 1981).
M. and O.–Thomas Mann and Norman Ornstein, eds., *The New Congress* (1981).

tween the branches and the consequences for the political system. Some have provided alternative models of executive-legislative relations or have analyzed the dynamics of expanding executive and declining legislative power. Others have argued that declining congressional power threatens the fabric of representative government. Still others have argued that presidential weakness, combined with congressional parochialism and disarray, have created a general crisis in governmental competence.

These works, and others like them, continue the long tradition in the United States of assessment and argument with respect to the proper roles and actual power of the legislative and executive branches. A related and overlapping traditional concern with the representative character of Congress is also present in the contemporary literature. With some exceptions, however, it has largely taken the form of empirical analysis of the character of representation. In large part, this research exists as a component of the literature that examines constituency-member linkages.

The contemporary literature that focuses on Congress as an independent variable is informative, enlightening, and sensitive to important aspects of change. As such, it has value for diachronic analysis as a source of information and direction. Nonetheless, contemporary research is also flawed by continuing vagueness in conceptualizing and measuring both role and performance, and in treating their interrelations. As a result, the contribution of current research to identifying and explaining change in a precise manner is deficient for reasons that go beyond its limited historical range.

The current state of analysis reflects a cardinal fact. Institutional analysis has lagged behind behavioral analysis since the advent of the behavioral revolution in the early 1950s. Our ability to handle questions that posit individuals, whether in small numbers or large aggregates, as the units of analysis is far greater than our ability to handle questions that posit institutionalized collectivities in complex environments as the units of analysis. Students of comparative and state legislatures have begun to confront this problem; congressional researchers must also begin to address it if diachronic analysis is to proceed.

Environmental Manipulation. Popular mythology and press accounts have long emphasized the primacy of self-interested motives and behavior among members of Congress. However, from the end of the Progressive period until the era of Vietnam and Watergate, this theme received little scholarly attention. Indeed, even in the literature on constituency linkages and impacts it remained submerged beneath other themes and concerns. In the 1970s, however, a number of scholars began to examine the manner in which members use organizational positions and resources for their personal advantage.

The two major studies are David Mayhew's attempt to explain congressional structure and behavior generally in terms of reelection motives (1974a) and Morris Fiorina's attempt to explain the growth of bureaucratic programs and power in terms of the desire of members to expand the resources they can deploy to bolster their reelection chances (1977b). These two studies, in turn, have stimulated detailed analysis of the causes and impacts of casework and have played a role in the increased attention given to the processes of allocating federal grants and contracts. In addition, scholars have begun to investigate the impact of member goals on committee behavior, conflicts between personal goals and institutional needs, and the exploitation of negative institutional standing for electoral advantage.

Yet many other aspects of environmental manipulation remain unexplored. Despite the increased number of indicted members, studies of corruption or ethics are sparse. Despite the increased power of the media, studies of the techniques and impacts of manipulating appearances or creating images for either personal or partisan advantage are few. Moreover, conceptualization and measurement of the complex patterns of motivation that actually exist and their pathologies and consequences in an organization, such as the Congress, remain primitive. In sum, then, this area of research is important, though underdeveloped. Organizations have an impact on their environments; they do not simply respond to them. This fact is of immense significance for the study of Congress as an independent variable. It defines an important area of research that also needs to be pursued diachronically, both in terms of historical range and analytic sophistication.

Defining a Research Approach

To improve scholarly capability for diachronic analysis, a research approach is needed which can overcome the major problems identified in our literature review. We shall draw on organization theory to define such an approach and illustrate it with examples where appropriate.

It is necessary first to expand the definitions of operations and performance that were offered earlier. In research which treats Congress as a dependent variable, operations may be conceived as the work processes involved in producing the outputs of the organization. So conceived, inquiry concerns the character and determinants of the key elements that compose and order these processes: norms, structural patterns (in the sense of prescribed distributions of task, authority, and power), forms of member and leadership behavior, and incentives. Performance may be conceived as the results or consequences of congressional work processes relative to organizational goals or objectives. So conceived, inquiry can proceed in various ways. It can be directed at the attributes or characteristics of output patterns, e.g., the degree of detail in lawmaking or the mix or balance between lawmaking and constituent service. It can also be directed at aspects of performance more abstractly defined, e.g., Congress' role in terms of function or power in the policy-making process.

In research which treats Congress as an independent variable, operations may again be conceived as the work processes involved in making decisions on organizational products or outputs. Our conception of performance, however, must be broadened. Questions of systemic impact are now central. Performance may thus be conceived as the results or consequences of congressional activity for systemic maintenance or enhancement. Primarily, this involves attention to the impacts of aspects of congressional performance, defined in terms of outputs or roles, on systemic integration, goal attainment, and adaptation to the broader societal environment. To some degree, however, it also involves attention to the impacts of aspects of congressional operations on the operations and performance of other units in the political system. This last point highlights a more general fact. In all instances, congressional impacts on systemic maintenance or enhancement are filtered through other environmental units, and these relationships pose difficult, but important, research questions. One further element of complexity must also be noted. Analysis of the Congress as an independent variable cannot simply assume service to system needs. Manipulation of the environment by the Congress must also be considered and assessed.

Analyzing Operations. In presenting our views, we shall begin with the determinants of operations and discuss this topic in far greater detail than either the determinants of performance or the analysis of impacts. This choice reflects more than the logical priority of operations to other topics of inquiry. Research on operations has been more extensive and fruitful than other types of congressional research. Thus whatever the problems and gaps that exist, it is far easier to stand on the shoulders of investigators in this area than in the others.

At present, the concepts and measures of dependent variables used in operational analysis are usually concrete. Reliance is placed on concepts such as party or bloc voting, leadership ladders, subcommittee expansion, and incumbency. Most of the measures applied are therefore also concrete. They generally take the form of an index or count of one kind or another. While statistical treatments of these measures are often sophisticated, the overall results are usually limited in their explanatory power. Concrete dependent variables suggest and are most readily amenable to concrete independent variables. Constituency interests, party balances, constituent service, and leadership traits provide some prime examples. It thus becomes difficult to rise above relatively mundane accounts of the effects of a few, familiar factors. In addition, researchers can easily be enticed into believing that the primary problems are measurement problems, e.g., bloc analysis.

In some cases more abstract terms are used to conceptualize dependent variables, ranging from broad notions, such as structure, process, and behavior, to more intermediate ones, such as member role or job, institutional norms, and committee strategies. However, explanatory power remains limited. The broader concepts are typically defined and applied in such vague and overlapping ways that their use and significance are largely ritualistic. The more intermediate concepts are usually defined and applied in such isolated ways that their utility is also limited. Abstract but isolated dependent variables suggest and tend to be treated in terms of separate and discrete independent variables such as member goals, output demands, or role orientations. Explanations of this kind, even if abstract, have limited ability to take interactive effects with other external or internal determinants into account. For example, member goals are affected by electoral results, party strength, institutional resources, and the ability of the party leadership to achieve policy goals. Their character and impact on matters such as job performance, institutional norms, or committee strategies are thus not simple but complex, and cannot adequately be explained without reference to a variety of conditions and factors.

What we need therefore are more general and integrated ways of conceptualizing dependent variables, combined with measures that operationalize key aspects of these concepts and permit tests of theories based on them. Some examples will illustrate our argument.

In analyzing structural change, we need to move beyond conceptualization and measurement of the dependent variables in terms of committee units, party units, and ad hoc groups to conceptualization and measurement in terms of the degree and forms or organizational elaboration, specialization, formalism, centralization, and so on. These concepts derive from a broad conception of structure as prescribed and regularized distributions of task, authority, and power. So conceptualized, they define generalized and related facets of structure (Cooper, 1977, in D. and O., and 1981). In analyzing change in leadership behavior, we must move beyond analysis of the selection, activities, power, or personal traits of particular leaders to analysis in terms of broader categories of function and style. Such categories must be derived from more generalized conceptions of the functions of leaders in complex organizations and the patterns of behavioral choice that define styles, e.g., types of incentives employed, orientations toward task or performance, reliance on formal mechanisms, orientations toward risk or innovation, and emphasis on personal distance (Cooper and Brady, 1981). In analyzing voting patterns at macro or collective levels, we must move beyond conceptualization and measurement in terms of party or bloc to conceptualization and measurement in terms or more precise categories of cohesion, such as highly factionalized nonpartisan states as in the early 1820s, highly unified partisan states as in the late 1890s, and fragmented partisan states as in the 1940s and 1950s (Brady, 1974; Cooper et al., 1977; Jones, 1968).

Conceptualization and measurement at this level will lead to a superior understanding of change. For example, structure conceived and operationalized in terms of centralization allows us to treat party and committee in a unified manner and makes distinguishing change from stability far easier. A focus on either party or committee as the unit of analysis encourages an emphasis on stability since both endure over time. Centralization, however, can readily be seen to have varied widely from the late 1890s to the 1980s. Similarly, voting patterns when measured in terms of a factionalized-unified continuum reveal change more readily than measures of party voting. Party voting as normally conceived will usually show considerable stability (Clausen, 1973), whereas patterns of cohesion or coalition will vary more widely.

If we turn now to the character of the independent variables employed in explaining change in congressional operations, the same points apply. The concepts and measures of independent variables are also defined in either concrete or abstract, but isolated, terms, e.g., constituency interests or attitudes on the one hand, and member goals or cue-taking on the other. In both cases more general concepts may be implicit, e.g., environmental constraints, incentive systems, or decision making under uncertainty, but particular features of these concepts are equated with the whole. The result is to limit explanatory power, especially with respect to change.

Improved conceptualization and measurement of the independent variables, however, require more extensive efforts than is true in the case of the dependent variables. What needs to be attended to is not only more general and integrated conceptualization of the immediate variables treated as determinants, but also identification and inclusion of the contextual parameters within which determinants produce effects.

To recognize the importance of conceptualizing and measuring interactive effects is only to recognize the need to deal with the conditions under which results occur. The impact of any particular independent variable has to be assessed with reference to the impact of other external and internal independent variables and the patterns that exist among them. To recognize the importance of contextual parameters, however, is to recognize something analytically distinct. It is to postulate that the ranges of interaction and impact are limited by certain basic and enduring environmental factors, such as the character of the elective system, the formal division of function between the branches, or democratic values (Cooper, 1975 and 1977, in D. and O.). In the social sciences, where units and entitles are voluntaristic, it is preferable to conceive of such factors as *constraints* or *parameters* rather than absolute fixed points. Thus, improving conceptualization and measurement of independent variables involves dealing with ranges or limits of variation as well as patterns of interaction among variable determinants.

To illustrate, it is well known that the structural characteristics of the House of Representatives have changed greatly over time. Yet our ability to explain such change remains limited. Polsby's analysis (1968) of institutionalization is a pioneering and sophisticated approach that has advanced our understanding. Nonetheless, it has three basic flaws.

First, Polsby's approach avoids rather than addresses the analysis of external impacts by deferring environmental analysis to a later stage of inquiry in which "causes" will be treated. Instead, it focuses on the analysis of boundaries, reduces this analysis to the question of whether boundaries are open or closed, and then identifies long career or tenure patterns as the primary criterion of closed boundaries. Such a strategy has high costs. On the one hand, it assumes that the impact of all external factors are mediated by a single aspect of linkage, which is in turn narrowly specified. On the other hand, it leaves the host of stable and variable environmental determinants that have been identified in the study of organizations generally in the state of an undifferentiated melange with little sense of how they should be identified or assessed (Miles, 1980, pp. 189-315).

Second, Polsby's approach avoids rather than addresses the analysis of relationships among facets of structure and between these facets and external determinants. Thus, though it defines structural variables in abstract terms, it focuses only on complexity, defined as a combination of organizational elaboration and specialization, and universal norms. These factors are regarded as the touchstones of institutionalization and other structural facets and normative patterns are deferred to a later stage of inquiry where they are treated as "consequences" of institutionalization.

Again, the costs of such a strategy are high. Complexity, as defined by Polsby, is an important facet of structure. However, to focus on it and treat all other aspects as consequences is to ignore the interrelationships that exist among broad facets of structure, such as specialization, centralization, and formalism. Organizational structures must provide for three basic needs, not just one: for integration and motivation as well as division of labor. Moreover, these needs conflict and must be mutually accommodated. As a result, while no one would deny that the degree of complexity affects the degree of centralization and formalism, it is also true that the latter two affect the former (Hall, 1977, pp. 130-93). Equally important, to focus structural analysis on the consequences of complexity is to treat the House as a closed system in all regards because of its degree of closure in a single regard. In fact, apart from their impacts on House career patterns, a variety of stable and variable environmental factors have direct and substantial impacts on all facets of structure and their interrelations. Indeed, in other organizational contexts even the relationships between career patterns and complexity or between elaboration and specialization are not as straightforward as Polsby assumes.

Similarly, to seek to characterize all the role orientations that underlie House structure in terms of only one of Parson's five pattern variables is also necessarily partial and inconclusive (Bershady, 1973, pp. 93-124). For example, take Polsby's prime example of a universal norm —seniority. Greater attention to seniority rather than party norms can be viewed as a change in the direction of universalism. But it also represents a change from performance standards to ascriptive ones. Thus, to assess the net benefits or costs for organizational viability, which institutionalization presumably fosters, we need a broader analysis of the relations among pattern variables as applied to organizational adaptation or maintenance.

Third, due to its partial treatment of both environment and structure, Polsby's approach has a number of serious biases. By focusing on the closed status of boundaries in terms of career or tenure trends and relegating environmental analysis to the identification of "causes" at some subsequent stage of inquiry, Polsby renders his analysis dependent on stability in the boundary conditions he identifies. Yet, as recent retirement and tenure trends indicate, there is nothing irreversible about twentieth-century career patterns. Indeed, it is also possible that the long-term rate of election defeats will increase. Nonetheless, Polsby's insights and explanations are tied to particular historical circumstances whose determinants and interrelationships remain unspecified. His approach thus provides no analytical framework for anticipating change in career patterns or assessing its significance in the context of other determinants. Similarly, both because of Polsby's assumptions regarding system closure and his focus on complexity and universal norms as the prime internal variables, his approach cannot deal with the range of structural variation that actually exists. For example, as recent experience indicates, organizational elaboration or differentiation can increase as career or tenure levels decline. Or to cite another example, norms of seniority or apprenticeship can decline as organizational elaboration increases. Finally, by equating institutionalization with complexity, Polsby obscures the full range of organizational needs that provide a basis for structure. A serious anomaly results. Since, as Polsby himself recognizes, complexity can result in decentralization, the House can be seen to progress in institutionalization as it becomes more fragmented, individualized, and chaotic.

These problems can be remedied if we alter and extend the presumptions of Polsby's theory of institutionalization in terms of the broader perspectives of organization theory. This, in turn, can be accomplished if we emphasize environmental impacts and view structural arrangements as patterned responses to basic, but conflicting organizational needs. Given such an approach, we can identify contextual parameters and interactive effects in the manner suggested above and define a more flexible, comprehensive, and powerful framework of analysis (Cooper, 1981).

The contextual parameters in question derive from fixed or stable aspects of environmental value, linkage, and work. The House is not an army, a business, or a hospital. Rather, it is a legislative unit in a democratic political system, which is itself organized in terms of a separation of powers, geographic constituencies, and plurality elections. As a consequence, the House is not free to accommodate its organizational needs through the variety of structural arrangements that are open or possible in other organizational contexts. Rather, it must do so within the following parameters or constraints: limited tolerance for hierarchy; limited ability to control the size or character of its work force; limited ability to control its workload or output goals; limited ability to rely on technology as a basis for decision making; and limited ability to identify or merge institutional and individual interests. These constraints result in important elements of stability, e.g., reliance on committee units for the division of labor and party mechanisms for integration.

The primary independent variables are also environmental. They relate to more fluid and less abstract aspects of environmental values, linkage, and work. Four categories of such variables are of prime importance: the scope and complexity of environmental demands for outputs; the character of executive roles and resources; the state of the electoral system, and especially the party system; and the state of less abstract aspects of democratic values that define the more precise content of broad values, such as majority rule, equality, openness, etc. These external variables provide the basis of change, e.g., the varying character and power of committee units and party mechanisms.

The structural impacts of these variables within the fixed parameters we have identified are complex. This is true because of the multiple paths of interaction among external variables, organization needs, and structural responses. For example, changes in workload pressures, party balances, or recruitment patterns affect the character of division of labor needs, integrative needs, and motivational needs at one and the same time. Similarly, particular structural responses to changes in these needs are not only constrained by contextual parameters and influenced by the states of other external variables; in addition, they typically have consequences for the character of all three basic organizational needs, not simply the one they are primarily designed to serve, and they involve a variety of reinforcing and limiting effects on future choices. Note the multifaceted causes, aspects, and impacts of subcommittee expansion in the past decade.

Despite these complexities, structural change can be conceptualized, operationalized, and explained in terms of an organization theory approach which emphasizes external determinants and organizational needs (Lawrence and Lorsch, 1967). Consider, for example, two critical facets of current House structure: the high degree of organizational elaboration and decentralization.

More than 600 legislative, party, ad hoc, and administrative units now exist in the House. This is an impressive number for an organization of its size. This facet of House structure is usually explained in terms of output demands or member goals. Yet, considered by themselves, these variables are insufficient to explain either present or past patterns. In other areas, organizations resist output demands and/or rely on vertical integration to limit horizontal proliferation. Hence, explanation of the state of organizational elaboration in the House requires the recognition of fixed parameters that restrict choice, e.g., limited tolerance for hierarchy and limited ability to control workload or output goals. At the same time, explanation also requires attention to the interaction and impact of a variety of environmental variables, treated within relevant parameters and at general or abstract levels. For example, in addition to the scope and complexity of output demands, the state of the party system and other aspects of electoral linkage are critical factors because of their impacts on integrative capacity and on the willingness of members to pursue goals in a collective rather than individualistic fashion. Prevailing interpretations of democratic values are important as well because of their effects in legitimizing or creating discontent over distributions of task, authority, and power. In short, then, it is not that output demands and member goals are irrelevant, but only that they must be combined with other factors and treated at appropriate levels of analysis.

Similarly, it is commonly recognized that the current House has become highly fragmented and individualized, that a high degree of decentralization prevails. Explanations vary, but generally focus again on isolated or discrete variables: the independence of members, subcommittee proliferation, or the impact of complex issues on cohesion. Here again attention to environmental variables operating within fixed parameters and treated at proper levels of abstraction is necessary to provide a viable framework for explanation. The manner and degree to which the House can concentrate authority and power are constrained in critical ways by limited tolerance for hierarchy, limited ability to control the work force or workload, and limited ability to identify institutional and individual interests. The degree of centralization can nonetheless vary significantly depending on the manner in which institutional arrangements and party mechanisms combine to concentrate rewards in the hands of leaders. The state of the electoral system, especially as it relates to the strength of the party system, thus exists as the primary determinant of centralization. However, increases in output demands which force greater organizational elaboration in response to division of labor needs weaken the capacity to centralize, while increases in executive roles and resources reinforce it, though at the cost of autonomy. There is, in addition, one other consideration that merits attention. Decentralized organizations are not necessarily fragmented or chaotic ones. Formalism (reliance on rules to limit discretion) can serve as a source of integration and in so doing compensate for weaknesses in centralized power. In the House, however, such capability is limited because of the value-laden character of its work and its inability to rely on technology to routinize decision making. Hence, the anomaly previously cited in Polsby's work can now be resolved. A House, such as the current one, which is both highly elaborated and decentralized is also necessarily a chaotic one.

Analyzing Performance. The analysis of performance occupies a mediating or linking position in the study of Congress. In part, the reasons for this are clear. In treating Congress as a dependent variable, the analysis of performance ties the analysis of operations to wider questions of significance relative to the institution. Attention focuses not on operations per se, but on the consequences of patterns of structure and behavior for the maintenance or viability of the institution as a whole.

In part, however, the mediating role of the analysis of performance is hidden and in need of clarification. Whether explicitly recognized or not, the study of Congress as an independent variable is also essentially a "performance" question, though at a different level of analysis. Consequences again are the focus of attention, but this time analysis centers on the impacts of congressional operations and performance on the wider political system. Analysis of performance in its various connotations thus links different aspects of the study of Congress to one another.

To clarify the manner in which the analysis of performance is integrally involved in the study of Congress is only to identify a basic problem, not to solve it. An involved and difficult question remains: how to conceptualize and measure performance both as a dependent and an independent variable in ways that will permit research to be rigorous and productive. Our views on this issue are quite tentative since past research on performance offers fewer guidelines or cues than past research on operations. Nonetheless, it is an issue that must be faced. Improving our ability to analyze performance is critical to expanding our knowledge and usefulness as students of Congress.

As in the case of operations, the analysis of performance has been approached in both concrete and abstract terms. Let us turn first to the analysis of performance as a dependent variable, to the analysis of performance in the sense of the consequences of operations for the Congress as an institution.

Approached concretely, performance is conceptualized in terms of familiar and traditional output or product categories such as lawmaking, oversight, and constituent service. Due to ambiguities and overlaps in these categories, however, measurement is more difficult and less common than with respect to operations. In addition, conceptual and measurement difficulties limit comparative analysis of mixes of these outputs or products. For example, the degree to which oversight restricts lawmaking, or constituent service limits both, remains unclear.

Approached abstractly, performance is usually conceptualized in terms of role relationships to other elements or units in the political system. At this level, however, conceptualization has been so vague and ill-defined as to produce only infrequent and limited operationalization, e.g., comparisons of statute pages to pages in the *Federal Register* as an indication of losses of power to the bureaucracy.

Though more extensive and difficult, the basic problems that exist in improving conceptualization and measurement of performance as a dependent variable are of the same order as those that exist with respect to operations.

First, we need to extend and refine our concepts and measures so that we can link familiar aspects of performance to a more general and inclusive set of concepts. In the analysis of performance as a dependent variable what this fundamentally involves is distinguishing "outputs" from "outcomes" and relating them to each other. This is no easy task as is evidenced by the high degree of uncertainty that now prevails over how to pursue it. For example, how does one measure the attributes or dimensions of lawmaking, oversight, and constituent service individually or in relation to each other; how does ont examine and verify claims that Congress has gained or lost power to the executive over some specified period of time?

If we assume that a promising strategy to follow is to treat legislative products as outputs and role relationships as outcomes, then both more sophisticated measures of outputs and greater specification of the concept of role are needed. To accomplish the latter goal, it is advisable to emphasize the power or relational aspects of role rather than its functional or substantive aspects since what is at issue in treating performance as a dependent variable is maintaining Congress' position or domain relative to other units in the political system.

Such a strategy accords well with recent developments in the organizational theory literature which emphasize power or dependency relationships in analyzing environmental linkages, e.g., the autonomy or scope of organizational decision making (Pfeffer and Salancik, 1978, pp. 39-111; Miles, 1980, pp. 374-83). Research thus could initially be premised on relating patterns of structure or behavior to types of products mixes and types of product mixes to changes in the autonomy or scope of policy discretion vis-à-vis the executive and the electorate.

Second, as in the case of operations, we must not only expand and refine our concepts and measures, but do so in a manner that facilitates diachronic analysis. Here as elsewhere, this means recognizing and including contextual parameters and interactive effects. Over time the relationship between operations and performance is not one-way. Rather, performance results alter environmental and internal expectations and in so dong affect structure and behavior. Thus, while we must presume that certain patterns of operations produce certain types of performance, we must also remember that performance affects operations. To cite a prime and familiar example, losses of autonomy to the executive redefine the roles of congressional party leaders. In sum, then, to approach performance as a dependent variable in relation to its significance for institutional maintenance is to approach it over time as adaptation and adaptation can and usually does involve changes in structure and behavior as prime forms of response to stress.

The analysis of congressional performance as an independent variable has been an even more perplexing and frustrating area of research. To a greater degree than in other areas, we are still grappling with the task of converting our understanding of Congress' impacts on the wider political system into something more than haphazard insights or presumptive conclusions.

The causes of this state of affairs relate to the problems of defining a frame of reference for treating performance at a different level of analysis. These problems, in turn, consist essentially of finding ways of transforming factors previously regarded as institutional outputs or outcomes into inputs which impact the system as a whole. Current attempts to resolve this difficulty take the form of emphasizing concrete outputs or abstract functional roles. However, each approach remains limited.

If performance is approached concretely in terms of outputs or products, the jump from analysis of patterns of output to assessments of systemic impacts is vast. In periods of crisis such gaps can be bridged more easily than in periods in which conflict does not threaten the viability of the system, e.g., the Civil War and the Depression. In isolated contexts they can be bridged by illustration or analogy on the basis of case studies. However, in all instances the evidence and conclusions are highly presumptive.

If performance is approached abstractly, it again is usually conceptualized in terms of role. However, when treated as an independent variable, emphasis falls on the functional or substantive aspects of role rather than its power or relational aspects. The strategy adopted is to assess systemic impacts by assessing functional performance on the basis of evidence regarding products or outputs. The problems that ensue are immense. They relate not merely to the empirical difficulties of analyzing role performance in relation to specific and concrete outputs, but also to the normative foundations of conceptions of role performance per se. Such conceptions, whether explicitly noted or not, rest on more basic conceptions of the character of role demands or expectations and these, in turn, must be defined in a highly prescriptive or normative manner.

Functional analysis is thus not value-free, but value-laden. The assessment of congressional role performance cannot proceed without prior definition of Congress' expected or proper role. And this requires interpreting democratic and separation of power values both in relation to the satisfaction of systemic needs generally and in relation to Congress specifically. In the United States the satisfaction of system needs is not simply a matter of managing conflict or maintaining trust, but of doing so in terms of processes that provide for representation and limited government. Similarly, Congress is but one of the key units involved and provides the locus of only one of the key processes. Hence, to analyze performance as an independent variable on the basis of role requires dealing with a complex mixture of normative and empirical elements and this is a task that both puzzles and deters congressional researchers.

It is therefore not surprising that analysis of Congress as an independent variable has been less productive than other areas of congressional research. To bridge the gaps that exist between outputs conceived concretely and systemic impacts, we need a more comprehensive and detailed theory of the political system. To employ the concept of role as an intermediary tool, we need to reintroduce normative analysis into institutional analysis. Let us hope that this can be done in a manner that improves on traditional institutional analysis. Hopefully, this can be done in a manner that improves on traditional institutional sophisticated analysis of the institutional implications of systemic values, and with greater sensitivity to the manner in which normative and empirical elements combine in frameworks of assessment. In any event, though the task is exceedingly difficult, the questions are too important to be avoided.

Defining a Research Agenda

We have called for the use of more general concepts to solve the problem of distinguishing change from stability in congressional studies. It is, of course, one thing to call for better conceptualization and measurement and another to achieve it. Improvements in both will be the result of the efforts of individual researchers to explain various aspects of congressional operations and performance over time. In this section we therefore suggest some topics which we believe have potential for increasing understanding of congressional change and stability.

The research agenda which follows will be divided into specific categories—environment, operations, and performance. These categories are not mutually exclusive. The main thrust of our argument has been that environmental, operational, and performance factors are interrelated. We also believe that the ways in which these factors are patterned pose the dominant theoretical questions and that the best diachronic studies will be those that can encompass and explain major facets of their interaction. The purpose of these categories is thus only to provide a framework for identifying sets of topics that illustrate the possibilities and benefits of diachronic analysis.

Environment. Three major environmental variables affecting congressional operations and performance are elections, congressional-executive relations, and congressional workload. Studies of congressional elections which focus on the extent to which elections are determined by local, state, or national factors are both important and possible to achieve. Stokes (1975, pp. 182-89) has devised a variance components model which separates out the variance attributable to each level. Scholars using this technique could show us how different elections influence the operations and performance of the Congress. Diachronic studies of the impact of elections on the constituent bases of the congressional parties would be useful and can be done. Surely, the constituency bases of the congressional parties vary over time on a number of dimensions, and have effects on how Congress works. Electoral studies mapping the extent to which presidential and congressional elections vary together can be done and should shed light on congressional performance.

Diachronic studies of congressional-executive relations and congressional workload are also worth pursuing. Congressional-executive relations have changed dramatically from the congresses of Joseph Cannon to the congresses of Franklin Roosevelt, and in turn from the Roosevelt era to the present. While such changes are difficult to quantify, thoughtful attempts to study these relationships would have a major payoff. Content analysis of newspapers is a potential remedy, and a preliminary analysis of the *New York Times* by David Brady shows that the Speaker of the House was far more prominent vis-à-vis the president at the turn of the century than is the case today. In a similar vein studies of the interactive development of the organization and staffing of Congress and the executive would shed light on changes in the role and power of the two branches.

The quantity and quality of workload are important components of the environment within which Congress functions. While recognizing this fact, political scientists have primarily emphasized increases in the workload over time. We can improve by paying greater attention to the causes and consequences of agenda change (Sinclair, 1981, in D. and O.). We can also begin to chart the growth of such variables as bills introduced, substantive versus procedural roll calls, increases in government programs and employees, and increases in the budget. Such studies will have to investigate carefully the relation between technical or institutional change and increased complexity in the economy and society. Research in this area can be guided by historical and economic works (Hays, 1975) which deal with major changes in the society and economy.

Operations. Diachronic operations research can be enriched by studies which compare committees, map patterns of partisanship, analyze coalitions within policy areas, and study different leadership styles. The study of congressional committees over time leads to a better understanding of the conditions under which committees change in structure and policy behavior. Such studies are quantifiable, particularly in appropriations related matters, and are rich in their potential for investigating change, e.g., the breakup of the Appropriations Committee in 1885 and recentralization in 1919. Studies of partisan strength are measurable from roll call data and can be expanded to include changing coalitions and issue dimensions. In addition, tracing use of the caucus over time can add to our understanding of partisan strength in the Congress and comparing the styles of different leaders can shed light on patterns of influence and effectiveness.

The diachronic study of congressional operations will also have to include such institutional factors as the changing contours of committee organization and power and changing procedures for referring bills, authorizing legislation, and making budgetary decisions. Failure to include institutional factors in the research agenda constitutes reification of the proposition that Congress is incapable of adapting to or influencing its environment. Moreover, as Polsby's pathbreaking work on institutionalization suggests (1968), operational analysis can profitably be pursued at general levels. Since certain normative and structural choices exclude others and also shape or constrain behavior, different types of Houses and Senates may be presumed to exist in different historical eras. Investigation of the character, causes, and impacts of such patterning in the House or Senate during particular periods of history pose a challenging research topic as does comparative analysis across periods or of the House and Senate in the same period.

Performance. Conceptually and quantitatively, the analysis of performance has proved to be somewhat elusive. Nevertheless, studies of change in congressional oversight of the executive, the mix between policy making and constituent service, and levels of productivity or success in the lawmaking process provide topics that can improve our understanding of performance as a dependent variable.

Given contemporary concerns, the study of the constituent service-policy making mix over time seems especially promising. The *Congressional Record* is rich with examples of nineteenth- and early twentieth-century constituency service. Cunningham's compilation of circular letters from late eighteenth- and early nineteenth-century congressmen also provides a large body of evidence on service-oriented behavior, particularly from territorial representatives (1978a). Moreover, it is not unreasonable to suggest that such studies will lead to an increased understanding of the nature of representation.

Diachronic studies of lawmaking can utilize the notion of policy congruence between constituent characteristics and policy positions to increase our understanding of both Congress and the policy process. Studies of policy making which focus on the number of votes required to pass landmark pieces or clusters of legislation would shed light on the conditions necessary for major congressional policy actions. Similarly, diachronic analysis of efforts to reform Congress so as to increase its effectiveness as a lawmaking body would increase our understanding of the relation between operations and performance.

Though the topic has not been entirely neglected, we still know little about patterns of oversight over time. Diachronic studies that examined the dimensions, forms, and impacts of congressional attempts to control the administrative process would shed considerable light on the character and determinants of congressional output mixes as well as on the ways in which Congress adapts to changing environmental relations or conditions.

Congress' impact on the wider political system poses the severest problems for researchers interested in diachronic analysis. Conceptualization and measurement in these areas are difficult because analysis involves normative factors and depends upon increased knowledge of the wider political system. The major area of concern relates to whether the policies passed by the Congress have systemic consequences, e.g., conflict management. Here let us suggest one possible research design which might contribute to an understanding of this difficult topic. Research that sought to compare congressional policy making during the Jacksonian, Civil War, Populist, Progressive, and New Deal periods would yield important insights into the role of congressional parties, committees, and leaders during eras when major policy changes were occurring. Moreover, the design permits variation on a critical variable —system collapse during the Civil War. While such studies will not automatically yield complete answers to the difficulties we have identified, a better understanding of Congress' effect on the system will promote improved conceptualization and more sohisticated research.

Studies of Congress' role in serving system needs must also be balanced and accompanied by studies that focus on manipulative behavior by members and parties for personal or group advantage. An impressive start in this type of analysis has been made by scholars such as Mayhew (1974a) and Fiorina (1977b, 1981, in D. and O.), but diachronic analysis of the forms and dimensions of such behavior would increase our understanding of the conditions that stimulate or repress it and the effects it has on patterns of operations and performance.

However scholars choose to study Congress over time, there will be a strong tendency to take quantitative data and apply econometric time series analyses. Our final thought is a caveat against the unexamined use of econometric analyses. Such time series techniques have been developed to study trends in markets. Markets are sensitive to seasonal and short-term variations, whereas political institutions—especially in the United States—are reactive in nature. Thus, when change does occur in political institutions and policy, it appears to be dramatic, with abrupt and immediate consequences. Techniques designed to measure change in sensitive mechanisms, like markets, may therefore not be appropriate to studying change in Congress. Researchers need to be aware of the assumptions of econometric analyses and to adopt such techniques only where applicable.

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Understanding Congress in the Long Run: A Comment on Joseph Cooper and David W. Brady, "Toward a Diachronic Theory of Congress"

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It has always seemed rather odd to me that it was necessary to exhort scholars to study an institution as it passes through time. By definition, an institution is something that grows up over a period of time. It is, therefore, axiomatic that its emergence and development over time will have to be investigated. This scholarly activity may be called something as mundane as "history," or it may be dubbed "diachronic analysis" as Cooper and Brady do, borrowing from the lexicon of anthropologists. But perhaps such exhortation is not so odd. In the social sciences, we cannot say that we possess very adequate theoretical groundings or perspectives on the basis of which to conduct inquiry into institutional emergence, development, or change.

An institution is known by its stability, its coherence, its uniformity, and its embeddedness in the society of which it is part. Institutions have time-honored norms and traditions, so that they are experienced subjectively over a period of time. Accordingly, an institution is durable; individuals involved in events related to it come and go, but the institution continues to exist unless concerted effort is exerted to alter or abolish it. Congress is very much an institution. It is hoary; it has not changed fundamentally for 200 years. It has withstood any number of attempts, most probably misguided or unconscious, to do it in. We expect that it will be here tomorrow. Those who are newly elected to it are not, and do not think they are, creating a new human organization, or reinventing a legislative body. Only political scientists reinvent the wheel; politicians understand time and space better. So, it is unimaginable that anyone could understand Congress today without having some idea how it got that way.

On the one hand, we know a great deal about Congress. It is, by an avalanche, the most studied legislative institution in history. If we were to pile up all of the relevant research on Congress, it would tower, have girth, look imposing. We know a lot about how members get elected, what their constituencies are like, how they succor their constituents; we now have a pretty deep understanding of how the machinery of the institution works, how committees operate, how leadership works, how coalitions come about; we understand a great deal about how Congress is linked to executive agencies, and how the president woos it and wins; and more. You might say that Congress, as it stands, has been studied enough. Students who take university courses on Congress, or the legislative process, and who spend time in Congress as interns, employees, congressional fellows, scholars-in-residence, or members, commonly say that Congress is "just like you said," that what they learned in school matched the reality. Maybe that is suggestive of the possibility that research efforts should shift their main course, leaving a skeleton crew to monitor ongoing events and fill cracks between the massive areas of knowledge. Maybe the study of Congress is "in crisis" because so much is known about it. Recently, not much interesting has been said on the subject, and the workers in the vineyard are now expending most of their energies focusing on small anomalies, unfilled cracks, and regions of investigation susceptible to systematic attack without the need for much conceptual equipment or theoretical justification.

On the other hand, ignorance about Congress, especially about Congress as an institution in the generic sense, is monumental. Few political institutions of its importance have as little history in the form of major literature about historical development. Cooper and Brady are trying to deplore this state of affairs, and recommend doing something about it. They want serious students of Congress to quit writing stories about "who-did-what-to-whom" on the Hill, or shouting bugbears and sky-falling about burgeoning staff, or fragmentation, or whatever, in order to consider more fundamental intellectual and research issues about the experience of Congress as an institution.

It is not true, of course, that Congress has no history, that there is no good historical research on Congress; Cooper and Brady do not claim this. Some general historical studies start, of course, as if Congress's nativity was at the Constitutional Convention and Madison, Jefferson, and Clay were the wise men. But there is historical research on Congress of real theoretical utility. I believe a start could be made at constructing "diachronic" generalizations with some of the existing work, and I think Cooper and Brady might have made richer use of the best work to make their case more convincing.

I believe, for instance, that an institution's development over time is affected very profoundly by how it got started, and we do have studies which tell us a good deal about how Congress was launched in the late eighteenth and early nineteenth centuries. Or, alternatively, I think a litmus test of an institution's elasticity and staying power is its ability to withstand upheavals in its external environment; we have studies of the impact of the slavery issue, and the loss of states during the Civil War which throw considerable light on the tenacity of the congressional institution. Or, again, I think an institution develops a culture which reveals itself in a manifold of highly subjective relationships among its most immediate members, so that it comes to have a very well-integrated group life; we know something about the life of the legislature when its members are in its chambers, and some of this work suggests spatial and interpersonal bases for the emergence of partisan structure. Or, finally, I think characteristic cleavages within an institution must reflect the contours of its external environment, if it is to avoid "parliamentary cretinism," as Marx called it, and run the risk of relegation or extinction. Brady's own work on congressional reflections of changing electoral cleavages in the 1890s illuminates this matter of institutional resilience and capability.

Nomothetic generalizations about a single institution are, perhaps, unlikely to come to us. It is a funny science of politics that studies only one institution, like Congress, holding dearly and unshakably to the thought that a theory of it, diachronic or synchronic, will unfold. Historians used to believe, and many may still, that generalization from historical study was impossible because each historical event was unique. I am reminded of an historian colleague at my university, a dear and precious scholar, who once remarked to me, when I asked him what he thought of the recent parliamentary election results in "his" country, that he had not paid attention, and that "if it didn't happen in the 1840s," he was not interested. Students of Congress often behave similarly. They treat Congress as if it were unique, and their implicit posture is that it can be compared to nothing else under the sun, except perhaps to its own previous incarnations. I do not believe that this will get us very far. Congress was not born without genetic inheritance, or clothes, a pristine monument to the enlightenment of the Founding Fathers.

Sometimes our work on Congress proceeds as if we thought some celestial visitor had come to teach us the intransmutable plan on which the institution was to have been built, and that our task was merely to study the main features of this "standing wonder"—like describing the sculptitude of Lady Liberty without wondering how she came to be there, in all her uniqueness and majesty. Congress is, of course, unique in many interesting ways. But it is the generic in a venerable institution which properly attracts the eye of the social scientist, and that we can attend to much better both in probing the institution diachronically, and in admitting it into the family of legislative institutions with which it should be compared. Congressional research would be more fascinating, and considerably more meritorious scientifically, if it extended its purview to the rear, and if claims about Congress as a representative institution were to march mainstream into the realm of comparative inquiry.

Cooper and Brady make a case with which I can agree in spirit. I am not so taken with their argument in all its particulars. Some of my misgivings go to detail, and are so menial as scarcely to deserve elaboration. I am not comfortable with their apparent belief that congressional development can be conceptualized as unilinear ("change is assumed to be additive. . ."). I do not feel at ease in speaking of Congress holistically as a dependent or an independent variable, even as a matter of expository shorthand. I think data about Congress will yield variables of the dependent or independent variety. Moreover, analysis of an institution over time surely will involve considering constants, as well as variables, since the leitmotif of an institution is its enormous constancy. The conventional paradigm of dependent and independent variables will not comfit entirely to a meaningful diachronic analysis of Congress.

But what is most disturbing, and most difficult, is that Cooper and Brady have not been able to unwrap the implications of diachronic analysis of Congress very far. It is, of course, one thing to call for a reorientation of the research agenda of a subfield in a discipline, and quite another thing to provide the beacon lights to guide the new directions. One of Cooper and Brady's instincts is to embrace "organization theory" for guidance, but this embrace is rather on the cold and clammy side. So-called organization theory has never informed legislative research very effectively, I think mainly because it was conceived with the bureaucracy, or the firm, in mind. The theory is not sufficiently general, so legislative organizations do not fit its rubrics well; the fit is strained at best. Many examples of the strain come to mind, but what bothers me most has to do with the constituent function of a legislature. Organization theory, which draws us to consider specialization, formalism, centralization, authority, etc., does not admit sufficiently to focusing upon what is the reigning property of a legislative institution—its grounding in a constituency. Moreover, unless I am mistaken, organization theory provides little in the way of an historical perspective, so its sustenance for diachronic analysis is fairly meagre. So, I am left mildly insouciant by the belief expressed by Cooper and Brady that Polsby's study of congressional "institutionalization" could have been improved by meatier attention to the insights of organization theory.

There is much talk of variables, structure, performance analysis, operations, and all that in the Cooper and Brady prescriptions, and I tend to warm to research prospecti which seem to offer conceptual clarity and direction. But, still a budding curmudgeon, I do not really get from the latter section of the paper a clear and workable notion of where diachronic congressional research should go. It has a tinny ring to me. Cooper and Brady do not want to sound like functionalists, and I think to avoid this they offer a rather starchy organizationalism. Maybe they do not mean to say so, but as I read it they think functional analysis is "value-laden," but organizational analysis is not. I don't believe it. However uneasy one may be about accepting it, I think it is ultimately impossible to talk about Congress or its development as an institution without using functional conceptions. All students of institutions are functionalists, whether they think so, or like it, or not. We just have not been able to figure out an adequate way to talk about institutions without functional connotations. We will not escape this limitation by converting to the language of variables.

Cooper and Brady, in the last section of their article, provide useful suggestions for future research directions. I am persuaded by much of what they say about mapping, untapped sources of data, and the extension of "single-shot" studies over time. On the one hand, their recommendations have a ring of feasibility; they are not so arcane that the workaday scholars cannot begin to tackle them. I hope many of their suggestions will be undertaken. But, on the other hand, it still is not as clear as it should be shere we ought to be headed, how we can understand Congress and other members of the legislative family better in the long run. Cooper and Brady have opened a discussion; they have not intended to relieve us of the need for all further thought. I am somewhat surprised at their parting shot at "econometric analysis," since surely none of us would want to use it in an unexamined way. I would have thought proper time-series analysis of appropriate quantitative variables related to Congress would likely be eminently respectable, and along lines urged by the Cooper and Brady critique. But their caveat in finale reminds me of my more general uneasiness. Perhaps contemporary research on Congress has, in a disturbing way, been short-changed. It might be argued that every serious study of Congress is implicitly diachronic, insofar as snapshots taken from time to time permit a kind of vision of Congress's timeline. After all, an institution like Congress is born in time, grows and adapts only over time. We may not always see it as time in motion, but even a quick blink gives us a specimen with time built in.

I think research along lines most abstractly promoted by Cooper and Brady is probably more difficult than they suggest. Analysis of institutions over time so as to extract important evidence, new insights, about their development, is not so easily within our grasp. In stable systems, institutions do not change much over long periods of time, so we face constancy, not variation, as our purchase. And that beloved chap, Edward McChesney Sait, surely was correct when he pronounced years ago (in a forgotten book entitled *Political Institutions,* 1938) that institutions "have taken shape like the coral islands, planlessly, by a series of minor adjustments that result from the more or less mechanical reaction of man to his environment." Sait had his eye on environment and experience in dissecting institutions, he understood that "the historical approach is indispensable" in studying institutions, he believed that "apart from their history and their environment political institutions cannot be studied with much profit." Cooper and Brady have their eyes on somewhat different things than Sait, but they are grappling with much the same intellectual problem. They deserve credit for plunging into the analysis of legislative institutions over time. Institutions may be arcane, and understanding them in time difficult. But since a good deal of contemporary research on Congress is narrow, and some is trivial, it is very timely that we should be reminded that more viable research directions could be taken. Congress is not unique; Congress is permeable, but it is unusually durable; institutions always embody time. So, our research should be more historical in the generic sense, and more comparative across space.

Studying Congress through Time: A Comment on Joseph Cooper and David Brady, "Toward a Diachronic Analysis of Congress"

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The difficulties in writing a general statement about what sorts of research people ought to do are very great. Consequently it ill behooves us to show anything but gratitude for the effort Joseph Cooper and David Brady have made to pull together a large and disparate literature in aid of the proposition that research on Congress recognizing the dimension of time is not a bad idea. Obviously, some of their examples assist the argument better than others, but in the main, the notion that it is useful to compare a contemporary institution with its previous self survives more or less undamaged. After all, most intelligible statements we can make about Congress are likely to be, in some way or another, explicitly or implicitly comparative, and the virtue of comparing any particular Congress with earlier Congresses is that many possible sources of "noise" in our findings—different constitutions, different party systems, different cultures—are diminished or eliminated.

The purposes for which scholars may wish to look at Congress over time are diverse—so diverse that it may be wise to withhold judgment about whether the literature surveyed in this review ought to be added up to one consolidated set of conclusions or as I suspect, to a varied menu of conclusions, about all sorts of things. Consider, briefly, a number of problems in which analysis of some aspect of Congress through time has figured:

1. *Congressional modernization.* Were internal pressures toward congressional modernization in the early twentieth century caused by party realignments of the 1890s creating safe congressional seats? (Price, 1975, 1977; Budgor et al., 1981).
2. *Presidential-congressional relations.* How and under what conditions did Congress relinquish to the president the publicity spotlight in national politics? (Cornwell, 1959).
3. *Purposive institutional change.* Do periods of congressional reform coincide with, or succeed periods of electoral instability so that they take place because there are an abnormal number of recently arrived, unsocialized members? (Peabody, 1966; Jones, 1968).
4. *Relations among congressional parties and factions.* How does the nationalization of politics and the strengthening of the Republican party in the South affect the ideological cohesion of the Democratic caucus? (Polsby, 1978). How do changing patterns of voting in the constituencies affect the distribution of safe seats among conservatives and liberals, southerners and northerners? (Wolfinger and Heifitz, 1965).

These studies are interesting not entirely because they are diachronic in their design. It is also the case that they contribute toward answering interesting questions about politics. Insofar as we want to know how and why things are changing, or have changed, time-conscious data-gathering is unlikely to go out of style. But it is our intellectual purposes that should determine our methodological strategies, not the other way around.

Because Professors Cooper and Brady pay my "Institutionalization" article the compliment of extended consideration, perhaps I can offer a few second thoughts of my own. (Actually, third thoughts: see Polsby, 1975). I begin with a brief response to their three main criticisms. Whether we believe that the neglect of the causes of institutionalization in the original article is a charmingly acknowledged and unavoidable limitation in an otherwise sterling performance (as I think), or something far more heinous (as my good friends suggest), the fact is, the article doesn't talk much about causes of institutionalization in the particular pattern adopted by the House. I do not think the article precludes such discussion, however. It has evidently not suited anybody's intellectual purposes as yet to follow up in the indicated direction. It will not be an easy enterprise.

Their second charge (a variant of the first) is that the article does not discuss in detail various components of institutional complexity and their external causes. This seems to me also largely true. Readers must decide for themselves whether the article has acted as a deterrent, preventing others from taking up this task.

Their third complaint is, if I understand them correctly, also a variant of the first: because the "Institutionalization" article says so little about the causes of institutionalization, it has little to say about causes of apparent reversals, downturns or shortfalls in concomitant variation among the various indicators chosen. This too is more or less correct, although I do call attention to the fact that the Civil War causes reversals in my indicators. To put the matter more abstractly: "Civil wars cause the breakdown of political institutions (cf. Polsby, 1968)." This may not be news to some readers. Relying on that off-chance, I join my colleagues in welcoming the exploration of further causes of changes in the pattern of institutionalization observable in the House.

The vulnerability of the "Institutionalization" article to criticisms such as Cooper and Brady have offered suggests that greater clarity is in order on at least one issue. In general, discussions of complex configurations of real-world events—like an institutionalized House—cannot reasonably aspire to fall readily under a small and compact set of covering laws. It is more reasonable to suppose that we might formulate more modest laws stating relations between variables two at a time. Over the short run, our desire to say something interesting about the entire configuration as it exists in history to some degree runs athwart the desire to contribute to the building of a social science by adding to the population of reliably observed bivariate relations. My hope that the original article would make a theoretical contribution was based on the idea that it would help in guiding the efforts of future students by pointing to features of the House that might repay closer examination because of their strategic location in the larger configuration. If I catch the drift of my colleagues' critique, this is not far from their own estimate of what the article actually delivers, and consequently do not begrudge them in their impatience with my failure to solve the next set of problems—namely, what causes organizations like the institutionalized House to develop in the way they do.

It is just possible that Cooper and Brady are asking this one article on institutionalization to haul more freight than it was designed to carry. My original notion was, I thought, straightforward enough: to examine some standard theories of institutional change that already existed in the literature to see if they would yield ideas to which I could attach empirical indicators. Because the indicators for the House were available and produced dramatic findings over time, the contraption worked. In place of apparently random, disparate, unrelated, but fascinating bits and snatches of detail, we could see that we had a syndrome of sorts: some trends that hung together in a theoretically intelligible fashion. These could be used in a number of ways in further discussion. For example, by showing how several seemingly unrelated organizational elements are related, we might make better guesses about what would happen if change occurred in the system, how a change induced at one point would ripple out to affect the entire institution. Do inroads into the automatic operation of the seniority system have any relation to the number of voluntary congressional resignations? Confident answers to questions like this are not easy to come by, but a better map of the institution, which aids us in seeing how organizational incentives are arrayed, surely helps (see also Polsby, 1974, pp. 67-148).

In company with many colleagues who study Congress, I believe that legislatures are badly understood as organizations because they are atypical as organizations in the flatness of their authority structure and in the internal legitimacy which they confer upon conflict, bargaining and coalition building. Thus it is necessary, as Cooper and Brady plainly see, nearly to reinvent organization theory for the purpose of understanding Congress. As this corporate scholarly effort proceeds, I believe we will continue to get some nourishment from the ideas floating about in the "Institutionalization" article—but not, perhaps, the Last Supper that Cooper and Brady crave.

In general, I agree with Professors Cooper and Brady that comparative and diachronic strategies are complementary rather than in competition in pursuit of the larger goal. Both constitute means for enhancing the stock of examples—at worst illustrations, at best verifications or falsifications—to which propositions about legislatures might speak. Whether the level of conceptualization at which Cooper and Brady choose to pitch much of their discussion will prove to be the right one for extracting the largest amount of portable knowledge from these examples is, however, to me an open question. My offhand guess is that a more selective use of Parsonian prescriptions is likely to inform my work than theirs over the long run. I am in no position to deny, for example, that "a variety of stable and variable environmental factors directly and substantially affect all facets of structure and their interrelations" (Cooper and Brady, 1981, p. 997), but as they rightly say, I do not hasten to affirm it either. The discussion of concrete characteristics of congressional organization—limits on hierarchy, limits on the capacity of the organization to control its own workload, limits on the capacity of the organization to impose norms on its members—seems to me to be headed in the right direction, though I was surprised to see that their analysis led them to the belief that the contemporary House was, by virtue of these factors, its organizational elaboration and its decentralization, "chaotic" (Cooper and Brady, p. 999). I think Cooper and Brady are right in suggesting that students have found it difficult to characterize congressional performance in ways that link to features of organizational structure, but I am unsure whether they have put their finger on the problem when they say (for example),

> Hence, to analyze performance as an independent variable on the basis of role requires dealing with a complex mixture of normative and empirical elements and this is a task that both puzzles and deters congressional researchers (Cooper and Brady, 1981, p. 1000).

These differences between us may boil down to differing conceptions of theory and its uses in social and political analysis. They may reflect different preferred vocabularies. Or they may reflect differing degrees of optimism about the possibilities for coordinating scattered research efforts. On the whole, I believe such coordination generally takes place, if at all, in response to something other than an explicit attempt to make a series of recommendations. All the more reason, I think, for students of Congress to applaud this effort by Professors Cooper and Brady.

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COMMUNICATIONS

Comment on Sullivan, Piereson, and Marcus (Vol. 75, March 1981, pp. 92-106)

TO THE EDITOR:

The article by John Sullivan, James Piereson, and George Marcus on the sources of political tolerance (1981) is their second recent contribution to the growing literature on attitudes towards unpopular minorities to appear in the *Review.* Although the more recent piece adds considerably to our understanding of the process by which people became tolerant or intolerant, one aspect of the article troubles me somewhat and should, perhaps, be discussed in the *Review:* their dependent variable, the "content-controlled" measure of tolerance itself.

I have no quarrel with the need to content control measures of tolerance. I would question, however, whether the notion of a content-controlled measure as presented by Sullivan et al. is quite as novel as they think it is and, secondly, whether their method of controlling is the best available. The technique used by Sullivan et al. is to supply respondents with a predetermined list of potentially unpopular groups and to ask them to select one of them that they most dislike; respondents are able to suggest any other group which they dislike even more than those on the list supplied. Tolerance questions are then asked about this most disliked group.

An alternate technique, which I reported in the *Review* in 1976, presents respondents with a list of groups, asks degree of like or dislike for each of them, and asks tolerance questions about each of them. The need for "content control" is explicitly argued in the 1976 piece, whose substance is referred to in both Sullivan articles; but Sullivan et al. make no mention of the methodological issues raised there and make no explicit defense of their own measure.

The advantage of the 1976 technique is twofold. First, because it asks all respondents about the same groups, it enables one to judge the likely mass reaction to groups actually likely to become the objects of popular tolerance or intolerance. Because Sullivan et al. ask only about least-liked groups, they cannot determine how citizens individually would react to groups they do not dislike least (or at all) or how the population as a whole would react to any group at all.

The second advantage is that the 1976 technique allows one to consider what I called the issue-relatedness of tolerance: the extent to which one's tolerance is related to attitude towards the group to be tolerated. It allows one to do this in two senses: first, observing effects on individual tolerance of differing degrees of like or dislike; second, observing the extent to which the population as a whole or different subgroups of it are affected by varying orientations, a procedure that permits comparison of levels of issue-relatedness of the subgroups. The perceived threat measure in the Sullivan et al. article hardly captures the same range of orientations, and it is in any case asked only about groups already identified as least liked.

The potential advantage of the technique used by Sullivan et al. is its "Is there anybody else you dislike more?" feature, which guards against the possibility that a predetermined list will exclude groups that any particular individual really dislikes. It has been claimed, for example, that my 1976 article suffers from a failure to include right-wing groups, a charge I consider completely justified. But such problems are hardly inherent in the technique itself. Furthermore, even with this failing the 1976 piece found that only 5 percent of its sample failed to dislike at least one of the groups listed, while the opportunity offered by the study described by Sullivan et al. to suggest additional disliked groups was taken by only 2 percent of his sample. It hardly seems to be the case that predetermined lists are incapable of identifying at least one group that everybody dislikes.

I agree entirely with Sullivan et al. that a content-controlled measure of tolerance is necessary; I agree entirely that the Stouffer, Prothro/Grigg, McClosky, Nunn et al. literature lacks such a control and suffers, therefore, from serious conceptual and empirical problems. What I do not agree with is that their particular measure is the richest or most flexible measure available. They have certainly not demonstrated this to be true. The question of *how* to content-control tolerance should at least be raised in the *Review.*

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Reply

TO THE EDITOR:

We agree with Lawrence that alternative techniques are useful when theoretically appropriate. Lawrence recognizes the primary conceptual point we make—the definition of tolerance requires that the person holds objectionable the group in question so that tolerance consists of setting aside that negative inclination. In game theory terms, tolerance presumes a cost, that of enduring an objectionable group. We do not claim our measure is superior to Lawrence's measure, only that it is more appropriate to our conceptual definition of tolerance.

We also included in our study investigation of responses to the second most disliked group, as well as some of the traditional Stouffer and McClosky items. A full discussion of these findings is reported in Sullivan, Piereson and Marcus (forthcoming, 1982). Lawrence's (1976) work was very important to our own work and his measures remain a useful way to investigate mass reactions to groups and issues. Each investigator ought to choose that measure most appropriate to the theoretical task.

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Editorial Note

This issue is my last as managing editor. It has been a rewarding experience. We managed 17 issues of the journal in Pittsburgh—from December, 1977, through December, 1981. I had the support of an outstanding staff and Editorial Board, and an APSA Headquarters sensitive to our every need. The book reviews were superbly managed by Paul Allen Beck and Holbert N. Carroll, each with their own fine advisory boards. As is noted in my final report printed in *PS* (Fall, 1981), thousands of members have been involved in the work of the journal—as authors, readers, and reviewers. I thank you all.

The following articles have tentatively been scheduled to appear in the March, 1982, issue:

Elaine Sharp, University of Kansas, "Citizen-Initiated Contacting and Social Status: Determining the Relationship and Accounting for It"

Barbara Romzek, University of Kansas, and J. Stephen Hendricks, University of Texas, Austin, "Organizational Involvement and Representative Bureaucracy: Can We Have It Both Ways?"

Youssef Cohen, University of Michigan, " 'The Benevolent Leviathan': A Study of the Political Consciousness of Urban Workers in a Context of Extreme Working-Class Dependence on the State"

Paul Allen Beck, Florida State University, and M. Kent Jennings, University of Michigan, "Pathways to Participation"

Nathaniel Beck, University of California, San Diego, "Parties, Administrations and American Macroeconomic Outcomes"

William Harris II, Princeton University, "Bonding Word and Polity: The Logic of Constitutionalism"

Charles S. Lindblom, Yale University, presidential address, "Another State of Mind"

J. Peter Euben, University of California, Santa Cruz, "Justice and the Oresteia"

BOOK REVIEWS

Political Theory and Methodology

Redefining Rigor: Ideology and Statistics in Political Inquiry. By Philip L. Beardsley. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. xii + 199. $18.00, cloth; $8.95, paper.)

This book addresses the question of why "conventional rigor" in the study of politics tends to preclude interesting, stimulating, and provocative work. Philip Beardsley claims that "unless and until political scientists begin to confront seriously the phenomenon of ideology as a problem in political inquiry, their work will not be genuinely rigorous." The present volume, he asserts, is "largely a critique of how rigor is viewed now," while a forthcoming volume offers a constructive alternative (p. ix).

This is a troubling book to review. Beardsley touches upon some exceedingly important problems of political inquiry which, as he rightly asserts, are usually neglected by empirically oriented political scientists. And, at least in part 1, many useful insights and arguments are brought forward. Nevertheless, this book was not yet ready for publication. It is too thoroughly riddled with fundamental problems to make a useful contribution to knowledge.

Beardsley's aim is *not primarily* "to expose ideological tendencies on the part of others." That task, he claims, "has already been accomplished." This book seeks, rather, "to treat ideology as a generic phenomenon." "How," asks Beardsley, "do certain general attitudes about rigor which currently prevail in political science interfere with the constructive handling of ideology?" And "why will the most fashionable specific methods in political science—various quantitative techniques—prove unable to resolve ideological issues?" Although these sound like good questions, this book is too confused, too unclear, to make much sense of the issues it attempts to address.

In chapter 1, for example, Beardsley seeks to place the concerns for this study "within a context provided by previous work by others that deals with or is relevant to ideology" (p. 1). He states his intention "to show that this work reveals four key elements of ideology." These are (1) *unavoidability,* (2) *pervasiveness,* (3) *actual or potential controversy,* and (4) *relative untestability* (p. 13). But none of these so-called key elements is substantive. They say nothing about what kind of thing or phenomenon possesses them. Furthermore, these same "key elements" could apply just as well to other things, such as language or *Weltanschauung*. They do not distinguish ideology. Nowhere in this book is an adequate (let alone rigorous) working sense of the word "ideology" established.

As this book's title suggests, Beardsley is very much concerned with "rigor." "There is," he writes, "a desperate need in political science for a conception of rigor" (p. 34). In chapter 2, he attempts to identify "ghostly standards of rigor which have had arbitrarily restrictive consequences" in political science (p. 42). But like "ideology" and other important words used throughout this book, the meaning of the word "rigor," itself, remains ghostly. It is never defined, let alone redefined, as the book promises to attempt to do. The word is used vaguely and idiosyncratically to refer to something like a "theory of inquiry." The author attempts to show that views expressed by leading political scientists (Easton, Eulau, Truman, and Almond) about the nature of political science attempt to deny "key elements" of "ideology." But since these "key elements" remain so ephemeral, the discussion gives the impression of boxing at shadows.

Nowhere in this book does Beardsley address concrete problems in empirical political science (statistical or otherwise) that would illustrate the consequences which he claims result from the "standards of rigor" he criticizes. Nowhere does he make clear what the "constructive handling of ideology" might mean, let alone show how this ephemeral task might be accomplished.

The chapters which deal with "ideologies and statistics" are so replete with problematic assumptions that space prohibits even touching upon them. Overemphasizing the distorting influence of *political commitments* in political science, Beardsley seems utterly blind to a host of philosophical problems bound up with the mathematical representation of political and social phenomena. Also significant about these chapters is their detachment—not only from actual studies in political science that make use of statistics, but also from the rest of the book.

This book is represented by its author as an important critique of contemporary political science and a programmatic proposal for an alternative. I do not, however, consider it a successful effort in either respect.

FRED H. EIDLIN

University of Guelph

The Origin of Formalism in Social Science. By Jeffrey T. Bergner. (Chicago: University of Chicago Press, 1981. Pp. xi + 162. $16.00.)

This is a lucid and compact book with a precisely formulated thesis that is both important and provocative. There are distinct problems in the execution of the argument, but they do not nullify either the significance of the issue addressed or the relevance of the material that is examined.

Bergner argues that social science will never achieve the kind of scientific maturity that it has set for itself as a goal. The inherent disability does not reside in the "inappropriateness" of a nomological social science but rather in the "formalism" of social scientific theory and its assumptions about "the relation between concepts and reality" (p. xi). This formalism is manifest in a concern with the "*form* of proper theorizing" and the "*form* of proper evidence" (p. 3) and in a neglect of the substance of theories. Bergner wishes to explain this formalism by tracing the "stages in the development" of social thought from the work of individuals such as Hobbes to the neo-Kantian philosophy of the late nineteenth century and its influence on the "founders" of modern social science.

While the goal in the early modern period was "to comprehend the human essence" or a timeless, universal, human nature, the philosophy of history, from the mid-eighteenth to mid-nineteenth centuries, emphasized the particular and unique and sought generality in terms of the meaning of the historical process as a whole. Bergner claims that "the thought which underlies modern social science has its origin in the critique of philosophical historical theory" (p. 61). Neo-Kantian philosophers such as Rickert and Windelband rejected the idea of the history of the West as an intelligible historical movement and transformed philosophy into epistemology. Their crucial assumption was that "the pure individuality of reality prevented any identification of concepts with the essential forms of reality's being, for reality was thought not to *have* any such essential forms" (p. 79). The neo-Kantians gave up the idea of knowledge as a representation of the form of reality and aimed at an internally consistent set of concepts that ordered reality in an arbitrary fashion based on the concerns and perspectives, of the observer. This gap between concepts and reality was the fundamental premise of the founders of modern social science such as Tönnies, Simmel, and Weber. Having given up the "ancient ideal of essential knowledge" (p. 137) and the belief in "natural objects to comprehend in naturally appropriate ways" (p. 105), social science, from the late nineteenth century to the present, has been characterized by an empty formalism, methodism, and theoretical pluralism which "makes cumulative knowledge a virtual impossibility" (p. 7) and neutralizes attempts to create a causal, objective, generalizing science. It is condemned to an eternal immaturity and "anarchy of substantive viewpoints, each of which is immune from decisive criticism or rejection upon theoretical grounds" (p. 134).

The principal problem with Bergner's argument revolves around his claim that he is presenting an "internal" historical account of the development of modern social science. Although the essay has a genetic chronological form, neither the "stages" in this development nor the connections between them are historically adduced in a convincing manner. And such crucial claims as that about the influence of neo-Kantianism on Weber are not supported by any detailed historical evidence or an extended analysis of Weber's work. Bergner's argument corresponds very closely to Leo Strauss's general claims about the origin and evolution of modern social thought, the manner in which positivism is rooted in historicism, and the relativity of knowledge and value in contemporary social science. Although the jacket offers the extravagant claim that the author demonstrates how the work of neo-Kantian social theorists "informs the whole of modern social science," there is little in the way of any attempt to establish concrete connections, and there is only a brief discussion of modern social science and its views about theory. An image of modern social science is juxtaposed to a general characterization of the founders who, in turn, are cast in the image of a certain strain of neo-Kantian epistemology. In all fairness, it should be noted that the author does not present the book as "an exhaustive account of the origin and meaning of all aspects of modern social science" (and in a little more than 100 pages such a project would be impossible) but as an attempt to provide a "plausible" explanation of some of the general characteristics of modern social science. But the argument is much too one-dimensional, and the claim that it is a historical argument must be carefully qualified.

In recent years, there has been a considerable amount of research on the relationship between late nineteenth-century philosophy and the origins of modern social science as well as attempts to develop definite historical arguments about the impact of these ideas on contemporary social scientific theory. Bergner does cite some of this material in the footnotes, but its correspondence with, and implications for, his thesis are not explored.

Bergner stresses, for example, the extent to which his argument contradicts the "misleading or altogether mistaken" assumptions of the "conventional wisdom" which "suggests that mainstream social science has its origins in 'positivism' " and particularly an English variety (p. 10). He argues, for example, that "all the serious originators of the fact/value distinction" were neo-Kantians and opposed to positivism (p. 125). First of all, it does not seem reasonable to argue that the conventional wisdom holds that the positivist influence on social science is unrelated to neo-Kantianism. Even some of the works that Bergner cites in support of this view largely argue that the positivist influence comes from the philosophy of science and is basically continental or German in origin. Second, Bergner neglects to explore, or consider, the complicated relationship between positivism, English empiricism, and the various schools of neo-Kantianism in the late nineteenth century and how these various intellectual tendencies and conflicts influenced social science in both the United States and Europe.

Despite these difficulties, the book calls attention to an important problem in contemporary social science and social scientific theorizing as well as to a critical historical period and a range of problems that are essential in understanding the development of modern social science.

JOHN G. GUNNELL

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Skills, Outlooks and Passions: A Psychoanalytic Contribution to the Study of Politics. By A. F. Davies. (Cambridge: Cambridge University Press, 1980. Pp. xiii + 522. $55.00, cloth; $14.95, paper.)

This book represents the most ambitious recent attempt to apply the insights of psychoanalysis to the study of politics. The writing is dauntingly learned. Citations include a dazzling array of scholars from a dozen fields. A. F. Davies displays an intimate familiarity with the literature linking psychoanalysis, psychiatry, and psychology to politics. A number of interesting, even important analyses emerge from the pages of this vast work. Yet I found it flawed in significant ways and ultimately disappointing.

The work is divided into three hermetically sealed sections. (The author himself admits that he has presented us with "three slim volumes bound as one"—p. xi.) In "Skills," Davies turns to Lasswell's famous typology of politicians (agitator, administrator, theorist) for use as his central organizing principle. He reminds us that politics is a world of work. Certain skills (agitating, administering, theorizing) are essential for those who wish to be successful at this work. He then summarizes a number of studies (e.g., those of Barber, Hoffer, Warner, Erikson) whose findings portray individuals who have successfully acquired one or more of these skills.

He has one major aim in this section: to show that other studies end by discovering Lasswell's original types all over again. But do they? To squeeze these later findings into Lasswell's early mold, Davies must resort first to developing "subtypes" from the original categories and then to adding another type altogether (the "balanced non-displacer"). We end not with Lasswell's original scheme, but with a sevenfold typology—a horse of a different color. And most readers will be able to think of types discovered in other studies which could not easily be accommodated by any of these categories. The Lasswell scheme, in short, does not appear as useful as Davies would have it.

In "Outlooks," Davies makes his most original contribution to the psychology-and-politics literature. His takeoff point is the complex typology developed by Robert Bales (*Personality and Interpersonal Behavior*) for classifying members of small groups as one of 26 personality types. Davies builds a solid case for this typology's value by the imaginative use of psychoanalytic case studies. He combs through widely dispersed materials to find real people who illustrate more or less precisely the Bales's types. Here one does not feel Davies is cutting and trimming to fit people into categories. Rather, he presents them whole and in so doing adds something to our understanding of each of the types, while reminding us that no flesh-and-blood individual can ever fit neatly into any intellectual cubbyhole.

Each reader will be able to think of objections to Davies's (and Bales's) work. Are the types clear enough? Are there not other types? Isn't the typology culture-bound? Once we know a person's type, can we actually predict anything about subsequent behavior? Nevertheless, the care and imagination with which Davies goes about matching obscure case studies to the Bales's categories suggests (if it does not finally convince us) that this is an important enterprise to which social scientists must now give serious attention.

In "Passions," Davies argues that social scientists have badly neglected an important topic: the enormous role played by basic human emotions—e.g., anger, envy, sympathy—in influencing political events at every level. His aim here is to direct our attention to, and encourage full-scale study of, the impact of "affects" on politics. To achieve his goal, he examines at length a number of specific emotions and suggests the variety of political consequences they are likely to produce.

He appears at his weakest—because most speculative—in this section. (And he admits the subject is a difficult one for systematic analysis.) Still, one interesting study—Davies's own—emerges from this discussion. He asked his students to keep a diary of the emotions stirred in them by political events over the course of a month. The results are intriguing. One can imagine a variety of fascinating research evolving from this approach: e.g., correlating group (or national) stability with the presence or absence over time of particular emotions felt by group members.

How can we put this book into perspective? With its strong and often arbitrary points of view, it cannot stand as an impersonal overview of work in the field. (For this, see Stone, Elms, or Greenstein.) Yet no major new theory is developed and tested, in the manner of a Christie or a Milgram. And the book's very structure is disturbing. Nowhere does Davies attempt to link his three sections in a theoretically compelling summary. Indeed, nowhere are we told just why this particular threefold division is used to organize his material.

What about the author's ostensible goal? Will the book encourage social scientists to pay more attention to how psychoanalytical studies can illuminate trends in the world of politics? Here some partial success is achieved. Beyond the high points already mentioned, the simple bringing to light of a variety of studies from different fields will help widely dispersed scholars become aware of their common research interests. It may suggest to many that the psychoanalytic perspective sharpens the meaning of traditional empirical studies of political actors.

Not all will be convinced, however. The behaviorally oriented, "harder" social scientists may be turned off by Davies's characterization of them as "phallic" (pp. 170-75). They will also be less than enthusiastic to see how little solid evidence there is yet to show whether toilet-training habits, breast-feeding schedules, and Oedipal complexes actually predict anything about adult political behavior. If one is not already convinced of the magical powers of psychoanalysis, one will probably not be converted by Davies's use of it here. For the outsider, psychoanalysis may appear once again as merely a brilliant ideological system of one-upmanship. (Why is it that anyone described in psychoanalytic terms, no matter who, appears "sick" in some deep and fundamental way?)

In short, this is a book for the committed. But for those who are committed, toiling in the vineyard of psychology and politics, it offers much to ponder and suggests some interesting avenues for future research.

OLIVER H. WOSHINSKY

University of Southern Maine

The Radical Future of Liberal Feminism. By Zillah R. Eisenstein. (New York: Longman, 1981. Pp. xi + 260. $17.95.)

Zillah Eisenstein has written this book in an attempt to demonstrate, primarily to liberal feminists and secondarily to radical and socialist feminists, both the contradictions inherent in liberal feminism and its radical potential. She contends that liberal feminists need to be more aware of the contradictions in their view of liberation in order to understand why certain of their demands will not be met by the contemporary state. She also argues that radical and socialist feminists should be more aware of liberal feminism's radical potential and seek to forge a political alliance with liberal feminists rather than dismiss them as middle-class reformers.

The bulk of the book consists of an explication of liberal feminism as developed in the writings of Locke, Rousseau, Wollstonecraft, Mill, Taylor, Stanton and Friedan. This explication focuses on the changing relations between the individual, the family, the economy and the state, as a consequence of the development of Western capitalism and the metamorphosis of precapitalist patriarchy.

In order to attack the patriarchal basis of feudal political rule, Locke differentiated between the "private" family and the "public" state, and left the patriarchal family intact. Rousseau defended familial patriarchy so eloquently that his writings became grist for the mill of the early liberal feminists: Mary Wollstonecraft, John Stuart Mill and Harriet Taylor. Rousseau felt that the family and the state were separate realms but equally important in creating a moral society. However, in order for men to be free, independent and self-sufficient citizens, women had to be subordinated to male authority in the family. And while Rousseau's Emile was educated to be independent, his Sophie was taught to be dependent and passive. Such a formulation contradicted liberal promises of individualism, educability and achievement for all.

Mary Wollstonecraft attempted to redeem liberal promises for her sex by extending to them equality of opportunity in economic and educational matters. Eisenstein argues that while such a notion provided women with the tools to reclaim their rationality and industriousness, it did not attempt to dismantle the patriarchal relations that enforced women's sexual and economic dependence in the first place. Although Wollstonecraft criticized Rousseau's ideas on the education of women, she adopted his separate-but-equal doctrine of home and political life. However, Wollstonecraft went beyond the liberalism of Locke and Rousseau to forge the beginnings of liberal feminism when she introduced the notion of women's oppression as a "sexual caste," or what Eisenstein calls a sex class. Herein lies the radical potential of liberal feminism: its collectivist claim to ending the sexual oppression of women as a group stands in contradiction to the liberal view of atomistic, isolated individuals whose problems are largely of their own making.

The feminism of Mill and Taylor was progressive in some ways but ultimately was constrained by "the ideology of liberal individualism" to exclude the working class. Eisenstein defines an ideology as a set of ideas which, while partially true, "serve to mystify the real relations of social power by presenting only part of them" (p. 10). The power relations masked by a liberal view of atomized individuals are those of patriarchy, economic class and race. Individualism can easily become, as it did for Mill and Taylor, privilege for the few who can achieve originality and education. Mill's contention that only the educated know "the public interest" hides the liberal equation of that notion with the private interests of the property-owning class. And Mill's idea of the state as a neutral arbiter of conflicting interests and the sole locus of "political" decisions disguises the ways in which the state promotes the interests of capitalism and extralegal forms of patriarchy.

Eisenstein compares the liberal feminism of two Americans—Elizabeth Cady Stanton and Betty Friedan—in order to demonstrate the lost radical potential within liberal feminism. The two differ in that Stanton's vision was informed by a radical feminist analysis of women's oppression as a sex, while Friedan explicitly dismisses the sex-class theory of power. Additionally, Stanton made self-consciously liberal arguments, where Friedan has no consciousness of her liberal political underpinnings and sees herself as a pragmatist. What both have in common, however, is a political strategy of demanding citizen rights and legal reform. While such a strategy was progressive in Stanton's day, today the state has come to accept such demands as the least threatening form of feminism. Friedan and the National Organization for Women have collaborated with the state in reducing feminist demands to legalistic ones and in isolating radical feminism as antifamily and manhating. Friedan's equation of politics with the public sphere excludes the family from political analysis, and her individualist theory of human rights prevents her from having to deal with questions of patriarchal social structure. She calls for equality of opportunity without questioning structures of male and class privilege in the workplace. She castigates women for accepting the feminine mystique without lodging its genesis in the particular needs of postwar capitalist production and consumption.

Eisenstein concludes with a discussion of how the "double-day" experience of women in the 1980s—that is, their double role as houseworkers and wage earners—has the potential to bring to the surface the radical potential of feminism as a mass-based movement. Married women in the work force may begin to question patriarchal dominance in the family and male privilege in the workplace which prevents equality of opportunity. Neither the liberal state nor the capitalist economy can allow for women's equality with men. It would cost employers billions of dollars to pay women equal wages and provide child care. The state cannot relinquish to women their power as reproducers of the species. It can grant juridical rights but it cannot afford to provide the material means to make it possible for women to exercise those rights as a sex class transcending economic and racial categories. While radical and socialist feminists know that the liberal state cannot deliver on its promises to women, they have to join with liberal feminists to push for those reforms that reveal the patriarchal nature of the liberal state.

The strength of this book lies in its meticulous elucidation of the liberal tenets of mainstream American feminism (individualism, formal equality of opportunity, separation of public state from private family matters, and faith in education and legal reforms). But certain theoretical tensions remain unresolved. Probably the most important of these has to do with the relation between the economy and patriarchy. While Eisenstein states that patriarchy operates autonomously from the economy (p. 20), she focuses on transformations in patriarchal relations as they accommodate the particular needs of the economy. One does not get a sense of the autonomy of patriarchy. The author's socialist feminist analysis portrays women's relation to the economy as more of a determining factor than the other social relations embraced by patriarchy: sexuality, childrearing, mothering and loving.

Eisenstein admits to the problematic nature of the notion of "patriarchy" (Ch. 2). While she is correct in rejecting both a universal ahistorical meaning of the term as well as a simple legalistic (father-right) definition, there is the danger of reification when one sees patriarchy as a "system" which "seeks to control" women, "tries to affect" their consciousness (p. 10), or "manipulates" the relationship between what is public and private (p. 25). Perhaps it is useful to distinguish between patriarchal ideas and those people who uphold a structure of male privilege: patriarchalists? In any case, this book makes an important contribution to the ongoing construction of a political theory of patriarchy.

However, the relation between such a theory and feminism as a political theory is less clear. While Eisenstein does not make the explicit claim that feminism is a political theory, it would have been appropriate to address this issue in some detail. She sees liberal feminism as a political perspective (p. 4), which no longer has a theory to inform its strategy (p. 3). It is not clear at what point a theory loses its status and becomes a perspective. Some liberal feminists do in fact use a political theory of liberal feminism to guide their actions. I agree that most liberal feminists do not see themselves as such, and this book fills a void in feminist literature that will help to remedy this situation.

While Eisenstein makes a convincing case for the contradictions inherent in liberal feminism, her argument about its radical potential is less carefully developed. Such potential has been discussed elsewhere by various schools of feminist theorists (anarchist, black, lesbian, radical and socialist), whose ideas receive cursory mention in this book in a way that will frustrate the uninitiated but no doubt will invite those concerned with mass-based movements to explore alternative feminist formulations.

JANET A. FLAMMANG

University of Santa Clara

The Political Philosophy of the Frankfurt School. By George Friedman. (Ithaca, N.Y.: Cornell University Press, 1981. Pp. 312. $17.50.)

This is a remarkably good study, one deserving careful attention by anyone interested in twentieth-century political and social philosophy. Friedman examines the "Frankfurt School" (mainly the thoughts of Adorno, Benjamin, Horkheimer, and Marcuse) in what I agree is even from their perspective the most appropriate context, namely, the "failure of modernity." For the Frankfurt School, this signified above all the failure of Marxism, and ultimately of the modern Enlightenment. While the above authors were in a sense part of the twentieth-century Marxist tradition, their experiences with Stalinism and Nazism directed them towards the modern critics of modernity, the so-called philosophical right. Although, according to Friedman, the Frankfurt school drew "its strength from the Right" (i.e., the Right Hegelians, Nietzsche, Heidegger, and even Oswald Spengler), he expounds the thesis that the Frankfurt School "could not live on the Left or the Right." Instead, they attempted to create a new philosophical stance out of the elements of both, one which would transcend the perceived failures of Marxism as well as the critics of modernity. In other words, they attempted to creatively combine the most ardently antimodern dispositions with a retention of the mssianic, teleological hopes of Marx. What Horkheimer called critical theory was intended, then, to afford the scholar-philosopher a scathingly critical distance from the brutalities of modern bourgeois mass culture, while keeping alive the hope for human salvation here on earth—the shedding of human alienation.

For Friedman, the Frankfurt School's unremitted adherence to the ideal of unalienated man, or their "too faithful adherence" to the central notion of modernity, was also their downfall. They only managed to "demonstrate the bankruptcy of modernity." In his conclusions, and after very often fascinating expositions of critical theory's view of the crisis of modernity and its search for a solution, Friedman briefly reveals his own sympathies with Aristotle and the standards of antiquity. If modernity is essentially the eradication of all human alienation, he rejects it because "life is by its nature alienated." What made the Frankfurt School unique was their "nostalgia" for virtue, for human excellence, for philosophy in an age of increasing mas brutality, while being unable to relinquish the dream for human liberation. An "unbearable tension" thus pervades much of their work, and exists between individual members of the school as well. Friedman elucidates in masterful fashion the various dimensions of the tensions and paradoxes found at the heart of the Frankfurt orientation. He thinks they knew they would fail. He also thinks it was "brave foolishness" that kept them going. That is a bit crude, I think, and will remain so until Friedman shows how they could have avoided either foolishness, or failure, or both, with the "standards of antiquity."

HORST MEWES

University of Colorado

Sozialismus: Ein Handbuch, Band I: **Von den Anfängen bis zum kommunistischen Manifest.** By Claus D. Kernig. (Stuttgart: Kohlhammer, 1979. Pp. 262. DM 44.)

Claus D. Kernig is perhaps best known among American political scientists as the editor-in-chief of *Marxism, Communism and Western Society: A Comparative Encyclopedia,* a multivolume work which explores the differences between socialist and Western countries. At the University of Trier, he is known as a dramatic and captivating lecturer, whose *Vorlesungen* are often crowded. Except for some entries in his encyclopedia, however, Kernig has not written any major work in the field. The appearance of the first volume of his three-volume *Lebenswerk, Sozialismus: Ein Handbuch,* changes this; and it is a welcome change.

Despite the suggestion of its title, Kernig's book is more than a simple look-it-up book about socialism. Rather, it represents a theoretical examination of the development not only of socialism, but of Western thought in general. Throughout his work, Kernig uses two theoretical constructs for understanding this development: subculture and causality. According to Kernig, "Socialism is the most powerful subculture of our time" (p. 11). It has evolved within capitalist culture, just as capitalism itself grew from a subculture within the medieval Christian culture to eventually become the dominant force in the West and, subsequently, in the entire world.

For a subculture to be successful in replacing the current system, it must possess seven characteristics: (1) have a simple promise, (2) influence the masses, (3) be morally constructed in a period of receptiveness to new ideas, (4) have potential for development and diversification, (5) be able to control the individual and society, (6) be able to control interactions, and (7) be capable of legitimizing power and building institutions (p. 26). In light of this definition of "subculture," Kernig traces the evolution of socialism.

Before he can deal with socialism per se, Kernig explores the development of capitalism within Christianity. It is here that the notion of causality figures in. In the classical and Christian cultures, human beings thought and behaved in a "heterocausal" manner. That is, they acknowledged the existence of causes which were not observable; they recognized the presence of a nonimmanent cause for all actions. Kernig cites Aristotle's four causes as an example of this heterocausal thinking. With the scientific theories of Newton and others, heterocausal understanding began to be replaced by a more "rational" conception of the relationship between cause and effect, which Kernig calls "isocausal." Human beings saw themselves as capable of controlling the results of actions through rational, scientific calculations. Human actions formed the sole type of cause. It is during the rise of isocausal thinking that capitalism developed and slowly replaced the Christian culture of the Middle Ages. "The old causality conception became limiting on human activities," Kernig says (p. 39). The prohibition against usury, for example, inhibited rational business transactions.

After discussing the establishment of capitalism, Kernig examines the origins of socialism as an alternative form of ordering society. He begins by exploring early forms of utopian socialism and carries the discussion through thinkers such as Owen, Blanqui, Blanc and Proudhon. In addition to elaborating upon the basic tenets of these socialist thinkers, Kernig demonstrates how their ideas conform to his concept of a subculture. He concludes his first volume by examining the background and content of the *Communist Manifesto.*

All in all, Kernig's *Handbook* is a fine addition to the scholarly literature on socialism. Admittedly, the book does have some problems. Is it really theoretically sound to argue that Western capitalism is now so widespread that it constitutes the only extant culture in the world and that "the future [of the world] lies in the inner possibilities of the capitalist system?" (p. 25). Is "capitalism," in fact, an adequate term for a culture, which Kernig seems to equate with modernity in general? In terms of causality, exactly what did Aristotle have in mind when he posited four types of causes? These questions go unanswered—perhaps because two volumes remain in which Kernig can elaborate upon contemporary culture and its relationship to socialism.

Despite these problems, Kernig's book serves an important theoretical function by exploring the question of causality. Although one is not yet quite sure what Kernig is advocating, it seems as though he is trying to give credence to the heterocausal mode of thought as fundamental in understanding reality. To the extent that he calls the reader's attention to the divine element in seemingly human actions, it reminds us of Herbert Butterfield's references to Providence or Eric Voegelin's discussions of Revelation, and to this extent, gives new meaning to a vital aspect of classical and Christian political thought—transcendence.

ANTHONY CLARK AREND

University of Virginia

Autobiography and Literary Essays. By John Stuart Mill. Edited by John M. Robson and Jack Stillinger. (Toronto: University of Toronto Press, 1981. Pp. liv + 766. $60.00.)

Volume 1 of the *Collected Works of John Stuart Mill* maintains the same high editorial standards that distinguish the 18 volumes John Robson and his colleagues have already published. In this volume, instead of the usual method of recording variants in the text, the authors present "parallel reading texts" of the early draft of the *Autobiography* and the final version that Mill prepared some 15 years later (the so-called Columbia Manuscript). The two texts appear on facing pages, and the blank spaces (sometimes whole pages) help identify the most extensive revisions. There are 2,600 substantive changes—so the editors report, and I am not about to challenge their count.

The cumulative effect of the changes on the tone of the final version should be quite evident even to those readers who do not care to count them. Partly in response to Harriet Taylor's suggestions (many of which the editors record), Mill produced a final version that is more impersonal and dispassionate than the earlier draft. Mill, for example, softens some harsh comments about his father and various associates, and omits completely the half page of (ungenerous) remarks about his mother.

For students of political theory, the most significant differences between the two versions consist not in what Mill revised, but in what he added. Only the final version contains Mill's well-known overview of his own theoretical development: the "greater approximation . . . to a qualified Socialism" and the shift from "pure democracy" to a "qualified form of it" (pp. 199, 259; but see pp. 238-39). Unacknowledged in the early draft, Tocqueville becomes in the final version an influential figure in Mill's political development (pp. 199, 201). The last manuscript also includes Mill's comments on his major works published after 1851, including *On Liberty* and *Representative Government,* and an account of his later political activities, including his parliamentary career (pp. 247-49). This section contains the passage, often overlooked by modern interpreters, in which Mill abandons his advocacy of the system of plural voting (pp. 261-62). Many other passages of significance for his political theory show no more than stylistic alteration; this is true of the "political" argument for democratic participation (p. 179) and the plea for a philosophy of history as part of any general theory of politics (p. 169).

The two texts of the *Autobiography* constitute only about two-fifths of this volume. Dominating the rest are 14 of Mill's literary essays and reviews, most, like "On Genius" and "Thoughts on Poetry and Its Varieties," from the 1830s. Two that did not appear in this decade have the most relevance for Mill's political theory. His first article in the *Westminster Review,* a critique of the rival *Edinburgh Review,* shows the style and commitment of an emerging philosophic radical, still in 1824 very much under the influence of James Mill (pp. 291-325). By the 1840s, Mill is himself contributing to the *Edinburgh Review,* publishing among other pieces a defense of his father's political theory, reprinted here as a "Letter to the Editor of the Edinburgh Review, on James Mill" (pp. 535-38).

This volume contains virtually all the texts that a scholar of Mill's *Autobiography* and literary concerns should need. It also contains more than most scholars, even *aficionados* of Mill, could reasonably want. The first of the nine appendices, for example, is devoted to Mill's "Juvenilia" (pp. 541-50): the opening pages of his first history of Rome, written at age six and a half, and his "Ode to Diana," probably written about a year later. Neither of these will greatly enrich our understanding of Mill or even enhance his reputation as a child prodigy, though the "Ode" may be taken as a portent of the dubious taste that allows the mature Mill to reprint several pages of fulsome poetry in his two reviews of Milnes in this volume. Even the editors concede that these pages may amount to "a small anthology of the world's worst poetry" (p. xlii).

Most readers will also pass by Appendix E, Mill's previously unpublished comment on Browning's *Pauline,* and Appendix F, short editorial notes from the *London and Westminster Review* (e.g., on the use of the plural pronoun by contributors to the journal). Other appendices should prove valuable, however—notably the list of Mill's early reading and the excellent bibliographic index, which includes in the usual style of the *Collected Works* all persons and works cited in the main texts. In a 47-page introduction, the editors provide useful information about the status of the texts and intelligent comment on the substance of the works.

Political theorists with any interest in Mill will be glad that we now have a definitive edition of the *Autobiography,* complete with the early draft and a scholarly apparatus of impeccable quality. But most are not likely to rush out and buy this volume, and they certainly will not want to assign it to their students. For about one-sixteenth the cost, students can purchase Jack Stillinger's paperback edition (Houghton-Mifflin, 1969), which except for two minor corrections is substantively identical to the text of the *Autobiography* presented in the Toronto volume. Research libraries and dedicated Mill scholars will have to add this volume to their shelf of the *Collected Works,* and then begin saving for the next of the eight volumes (including a cumulative index) that will complete the set.

DENNIS F. THOMPSON

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Theories of Imperialism: A Critical Assessment of the Various Interpretations of Modern Imperialism. By Wolfgang J. Mommsen. Translated by P. S. Falla. (New York: Random House, 1981. Pp. x + 180. $9.95.)

Wolfgang Mommsen, a German historian, has transformed a set of lectures first presented at Amsterdam University in 1970 into a highly readable and, for the most part, excellent short survey of theories of imperialism. (The translator, P. S. Falla, should be commended as well.) There are four major chapters. The first three cover theorists of late nineteenth-century imperialism and the fourth is devoted to theorists of neocolonialism and underdevelopment in the mid-twentieth century.

The first chapter, after a brief nod to theorists of the power politics school, is devoted to the work of Hobson and Schumpeter. The second is concerned with a wide spectrum of Marxist interpretations. Reviews of Hilferding, Kautsky, Luxemburg, and Lenin as well as a discussion of the decline of Marxist theory in the Stalinist era and the minimal contribution of the Maoist variant are presented. The third substantive chapter discusses the contribution of more recent theorists of imperialism and focuses for the most part on the debate within British historiography on the causes of British imperialism.

These three chapters are lively and excellent. Those unfamiliar with theories of imperialism will find this an excellent place to begin. Those already familiar with the subject matter will find the chapters an excellent refresher course and a chance to compare their insights with the trenchant assessments of Mommsen. Unfortunately, I cannot rate the final section of the book so highly. In his discussion of neocolonialism and underdevelopment, Mommsen gives rather short shrift to the *dependencia* approach to the study of political economy. Since the chapter is a rather brief survey of a vast literature, it collapses much of the literature covered and ignores much of the lively debates among the participants Mommsen does in fact discuss. More importantly, he excludes from consideration, among others, one of the most important of the *dependencia* theorists, F. H. Cardoso, and spends little time examining the debates within the *dependencia* and underdevleopment approaches.

In short, Mommsen sees the virtue of this entire line of argument as residing in the fact that it disturbs our complacent assurance regarding the overall positive effects of the Western development process rather than in its merits as a scholarly argument. He thus does not dwell on its strengths and weaknesses in any depth.

His analysis of neocolonialism and underdevelopment is also marred by an apparent lack of familiarity with the present international political economy. He writes, for example (p. 146), as if there is no First World investment in the Third World outside the raw material sector. This ignores the phenomena of the spread of multinational corporations and the rise of the newly developing countries, and thus much of the dynamics of the past few decades.

A final criticism is that the concluding chapter is far too short and lacks positive specificity for suggested improvements or development of a theoretical model with which to discuss imperialism as a problem of international politics. Nonetheless, I would recommend the book, if only for its treatment of late nineteenth-century imperialism.

MICHAEL STOHL

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On Justifying Democracy. By William N. Nelson. (Boston: Routledge & Kegan Paul, 1980). Pp. ix + 176. $16.95.)

"Democracy theory" is ambiguous. In the broader of its senses, it characterizes a particular ideal of the good society—roughly, one that is in some fundamental way both liberal and egalitarian—and the principles of legislation appropriate to it. In the narrower sense, democratic theory establishes a particular kind of procedure for political decision making—again roughly, one in which there is an institutional connection between popular preferences and authoritative decisions.

The focus of William N. Nelson's brief study is resolutely limited to democratic theory in the narrower sense. His concern, as the title indicates, is the moral justification of democracy, or the reasons why one might think it a good thing that political decisions be made democratically. The limited focus of Nelson's book is a virtue. The major recent works of political philosophy (for example, those of John Rawls and Bruce Ackerman) have tended to concentrate on broad, substantive issues of political morality while treating questions about the structure and justification of decision making procedures in a relatively superficial manner. For the most part, such attention as has been given to the latter questions may be found in specialized and even technical literatures. As a result of these two trends, democratic theory in the narrower sense has become fragmented, and the underlying philosophical problems have been blurred. Nelson's critical study of several approaches to democratic theory restores the unity of his subject by showing that each approach involves common philosophical issues of quite general interest.

By far the major part of Nelson's book is devoted to critical essays on each of four familiar views about the justification of democracy. Briefly summarized, these views hold that democratic procedures are morally desirable because: (1) they are *procedurally fair,* guaranteeing to each citizen an equal opportunity to influence political decisions; (2) they are necessary (if not sufficient) if a society is to be *participatory,* that is, to involve its citizens regularly in discussion and deliberation about public affairs; (3) they are required by the principle of *popular sovereignty,* which holds that government must follow the will of the people; and (4) they are more likely than any other type of procedure to produce legislative outcomes that maximize a society's aggregate level of preference satisfaction (Nelson calls this the approach of *economic theories* of democracy). Nelson's critical remarks are informed and judicious throughout, although the texture of his commentary is somewhat uneven. He is especially effective in discrediting views holding that democratic procedures are desirable solely by virtue of their formal properties, and in exposing the incoherence of views resting on the principle of popular sovereignty. On the other hand, the critique of economic theories is untidy and, by the standards of the genre, relatively unsophisticated.

The two concluding chapters present the outlines of Nelson's own theory, together with a penetrating discussion of the relationship of the problems of institutional justification and of political obligation. Like J. S. Mill's, Nelson's theory is instrumental: he argues that democracy is morally desirable because it is more likely than any alternative to produce just legislation. In support of this view, Nelson sketches a theory of just legislation, and he notes (although he does not give much evidence for) the important empirical premises on which the view depends. In outline, Nelson's theory appears to be more plausible than those criticized earlier, but many readers will prefer to withhold judgment until a more complete formulation and defense is provided.

The book has several shortcomings. First, the critical and constructive portions of the text are insufficiently integrated, and too much emphasis is given to the criticism of other theories relative to the attention given to the development of Nelson's own view. The result is a book that will give political theorists an unusually perceptive survey of views about the justification of democracy, but one unlikely to attract the broader attention that its positive thesis deserves. A related problem is that Nelson does not discuss the application of his theory to any of the various contemporary political controversies to which it is relevant (for example, those involving compensatory redistricting or party nominating processes). Greater attention to issues of this nature would have made clear why the problem of justification has practical as well as philosophical significance. Finally, in a book that is self-consciously interdisciplinary in its sources, it is surprising to observe that some widely read recent books and essays in democratic theory (for example, works by Jane Mansbridge, Dennis Thompson, and Michael Walzer) are not considered at all—despite their clear relevance to one or another of Nelson's principal concerns.

These are hardly crippling difficulties. *On Justifying Democracy* is a valuable addition to the scholarship on a neglected portion of democratic theory. It succeeds at once in bringing order to an area that has fallen into intellectual disarray, and in formulating the outlines of a theory of democracy that effectively challenges many of those now prevalent.

CHARLES R. BEITZ

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Political Concepts: A Reconstruction. By Felix E. Oppenheim. (Chicago: University of Chicago Press, 1981. Pp. x + 227. $22.00.)

Readers of *Dimensions of Freedom* and *Moral Principles in Political Philosophy* will encounter few surprises in Felix Oppenheim's latest book. Oppenheim continues to sponsor early Wittgenstein against late Wittgenstein, to uphold the strict separation of facts and values, and to espouse a "noncognitivist" view of moral language. He seeks to develop a neutral analysis of political concepts on which "political scientists with differing normative commitments can agree" (p. 1), and which are, accordingly, suitable for "scientific communication" (p. 179). He vigorously rejects theoretical orientations that deny the validity or importance of his enterprise. The beliefs that political concepts inextricably fuse empirical and normative considerations, that they are "essentially contestable," that they are relative to varying individual and cultural self-understandings, and that they cannot without loss of meaning be detached from general theories—all come in for extended criticism. In the chapters of the book not devoted to methodology or self-defense, Oppenheim brings his analytical perspective to bear on four key political concepts: power, freedom, egalitarianism, and interest (self-interest as well as the public interest).

The criterion of scientific communication guides Oppenheim throughout this endeavor. Scientific concepts must be clear and distinct, without ambiguity or inconsistency: the "first requirement" of a conceptual scheme is to "exclude the possibility of deducing statements from its definitions that are logically contradictory" (pp. 180-84). At the same time, political concepts (unlike those of the natural sciences) must remain as close as possible to ordinary language (pp. 179-80). It is for this reason that Oppenheim characterizes his overall project, not as the construction of an "ideal language" in the manner of classical positivism, but rather as the reconstruction of ordinary language, i.e., the minimal purification required to render it precise and noncontradictory.

Oppenheim's efforts can be evaluated on two levels: the utility of specific definitions, and the viability of his overall project. On the first level, the results are mixed. Some of his proposals (notably, the distinctions among several kinds of freedom) are illuminating. But others seem arbitrary and counterintuitive, even contradictory. For example, he insists that influence and power are not analytically linked to the intentions of those who exercise influence or power, for these concepts must be broad enough to cover unintended effects (pp. 45-46). Yet he affirms that "a government exercises no influence over those who break the law" (p. 13), although it is almost always true that the existence of the law has unintended effects on the *way* individuals act as they break it. Few criminals, however confident, proceed as though the law simply does not exist. Another example: punishment is defined relative to the intention of the individual who metes out punishment, not to the response of the individual who receives it. Thus, *R* is punished by imprisonment "even if it so happens that he is pleased to be in jail" (p. 18). Oppenheim's reasons for severing the analytical link between punishment and *R*'s loss of welfare are, to say the least, unpersuasive.

Another example: Oppenheim asserts, against Dahl, that there can be power without causation. If *P has* (without *exercising*) power over *R*'s not doing *x*, *R*'s not doing *x* is not the causal result of any action *P* does perform (pp. 32-33). But this appears to contradict the definition of "having power," all of the components of which involve some causally effective act on the part of *P* (p. 21). Again: Oppenheim distinguishes, quite properly, between unfreedom and inability. Unfreedom requires that inability be "caused, at least in part, by another person or jointly by several individuals or a group, intentionally or not" (p. 54). But if only the presence of human causality, *without regard to intention,* is relevant to unfreedom, then I am unable to understand how Oppenheim can go on to insist that whether unemployment under capitalism is or is not an instance of unfreedom depends on whether it is seen as the result of "institutions" and "anonymous causal factors," or rather "specific persons or groups." For surely institutional action is human and causal, although it may not be intentionally or conspiratorially directed toward the result (e.g., unemployment) it engenders. I do not mean to preclude the possibility that Oppenheim can resolve these and the many other specific difficulties this book contains; I do not see how they are to be resolved on the basis of the considerations he has so far presented.

On the general level, there are additional difficulties. Oppenheim espouses an instrumental conception of rationality, relative to each actor's "own intrinsic preferences"; there are no criteria of rationality "when it comes to ultimate preferences" (p.. 125). But, he declares, a threat becomes coercive when it is so severe that "any *rational* actor would give in" (p. 15). If, as it appears, he here defines rationality relative to a specific end—avoidance of death or great suffering—he both violates his instrumental view of rationality and renders "irrational" the decision of, e.g., imprisoned resistance fighters not to betray their comrades in spite of threats of torture or execution. Oppenheim asserts, against MacIntyre et al., that his conceptual explications are not culturally specific but rather universally useful, because they are structural explications that abstract from all specific perceptions, intentions, beliefs, and rules (p. 186). But he goes on to describe his project as the explication of "*our* basic political concepts" (p. 188; emphasis added), leaving open the possibility—at the heart of MacIntyre's argument—that the concepts "we" employ are aspects of our cultural particularity. It is hardly decisive, or even relevant, for Oppenheim to insist that "conceptual relativism is an obstacle to scientific explanation" (p. 186), for it is the possibility or appropriateness of such explanation in politics that is at issue.

I must note, finally, that Oppenheim's response to a difficulty he himself raises—that concepts unrelated to an overarching theoretical structure are bereft of real meaning and utility—is not terribly persuasive. Throughout this book, the reader is compelled to wonder whether the project of forging, one by one, loosely related analytical tools, ready for use by theorists and empirical researchers alike, can possibly be productive. We cannot easily evaluate the significance of an individual definition unless we see how it comports with a larger theoretical whole. It is regrettable that Oppenheim does not give us a clearer sense of how the specific concepts he analyzes can be so linked.

This review has emphasized the negative, because the virtues of this book will be apparent to even the casual reader. There is much of value in the rigor of Oppenheim's approach, which compels us to reexamine our unthinking use of ambiguous terms. And Oppenheim is to be admired for his simple clarity, unswerving determination, and utter indifference to changes of intellectual fashion.

WILLIAM A. GALSTON

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Anarchism: A Theoretical Analysis: By Alan Ritter. (New York: Cambridge University Press, 1981. Pp. vii + 187. $27.50.)

What are the central doctrines to be quarried in the canonical writings of the classical anarchist thinkers—Godwin, Proudhon, Bakunin, Kropotkin? By what arguments are these doctrines maintained? How do they fare in light of contemporary moral theory, economic knowledge, and political practice? Is the conventional picture of the anarchist as a Luddite egalitarian and arch-libertarian really on the mark? Answering such questions as these is the main task of Alan Ritter in this book, perhaps the first (certainly the first in English) in which anarchism is given a serious and careful theoretical assessment.

Let me say straightaway that Ritter succeeds remarkably well, thereby redeeming anarchist political theory from the undeserved neglect by serious critics it has for so long endured. Not for Ritter another historical review of texts, ideologies, and biographies of the leading anarchists and their dubious impact during the past two centuries. Although he does use extensive quotations from the sources, they are always in the service of setting forth and testing the theoretical framework he develops. The result is a focused, original, and illuminating book.

The central tenet of anarchism, according to Ritter, is to be found in the attempt "to combine the greatest individual development with the greatest communal unity" (p. 3), to achieve "the sovereign value of communal individuality" (p. 134). The early part of the book (Chs. 1-3) is devoted to establishing the centrality of this tenet. By reference to it, Ritter later contrasts anarchism to socialism and liberalism (Ch. 6), as well as to egalitarianism (pp. 76-82), and shows that it cannot be reduced to any of these three, even though it shares important convictions with each. Likewise, it is in terms of this tenet shared by the four anarchist apostles that their many divergencies on tactics and strategy can be assessed (Ch. 5).

As an illustration of Ritter's argument, consider his views of the anarchist's position on punishment (pp. 72-76). Anarchists are not so naive as to believe that perfect compliance with sensible norms can be expected once the threats currently attached to the criminal laws are superseded by less coercive incentives. What, then, are they to do about deliberately harmful conduct? Ritter takes the line that (a) anarchists are entitled to "rebuke" malefactors (p. 69); and also that (b) rrebuke is a form of punishment, albeit a mild one not likely to cause resentment (p. 75), even though anarchists have misled themselves and others by failing to recognize this (p. 73). Much here turns on whether rebuke really is a mode of a punishment and whether both rebuke and punishment involve "suffering" (p. 74). Ritter thinks they do, though he concedes somewhat grudgingly that Godwin and his successors were of a different persuasion. So, he concludes that instead of repudiating all forms of punishment as unjustified, the true anarchist regards "punishment, like authority, far from being at odds with anarchy, [as] one of its integral parts" (p. 76).

Two comments. "Integral" here is too strong; "incidental" would be more accurate, because ideally the practice of rebuke as well as the threat thereof will be at a minimum in the anarchist's society, thanks to the effectiveness of voluntary compliance, the internalization of norms, and the adequacy of milder forms of censure. As for whether rebuke is a form of punishment, and whether being punished *must* involve undergoing suffering, Ritter writes as if each is a foregone conclusion. I am unconvinced. Smith may rebuke Jones with the intention that Jones suffer, and this may even be the normal case. But Smith cannot be said to have failed to rebuke Jones merely because Jones is so thick-skinned or contemptuous of Smith that he (Jones) does not suffer any shame or remorse. The same can be said of punishment and suffering.

This and other minor points aside, Ritter's analysis is penetrating, and even where unconvincing (as above), it points up the issues. His singlemindedness in the pursuit of his argument, coupled with his thorough familiarity with both the primary and secondary literature (one will look in vain, however, for any reference to the recent anarcho-libertarian views of Robert Nozick) sheds new light on the distinctive features of anarchism that for too long have been misrepresented, misunderstood, and misleadingly interpreted.

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Simulating Terrorism. By Stephen Sloan. (Norman: University of Oklahoma Press, 1981. Pp. xviii + 158. $5.95, paper.)

The problem of international terrorism has in recent years seized countless headlines in the world's newspapers, spawning what is fast becoming a separate field of academic inquiry. The Reagan administration has declared the suppression of terrorism to be the cornerstone of its foreign policy, and the identification of the causes and facilitators of international terrorism has generated a lively debate within governments, private research organizations, and universities. But while more ink than blood has been spilled as a result of terrorist depredations, rare is the treatise which can boast of practical merit. While the discipline remains embroiled in definitional disputes and contemplating methodological navels, the death toll mounts.

However, a courageous few, while realizing the underdeveloped state of our knowledge of terrorism, have nonetheless attempted to adapt the tools of political science to combat terrorism. One pioneering effort has been that of the Study Group on International Terrorism at the University of Oklahoma, which has concentrated its inquiry on the taking of hostages. *Simulating Terrorism,* part cookbook, part substantive preliminary report, illustrates how the study group combines quantitative analyses of 111 international terrorist events, case studies, all-man gaming, and thespian techniques to train antiterrorist forces in the U.S., Korea, New Zealand, and Panama.

Stephen Sloan gives a step-by-step set of instructions for conducting these simulations, discussing the stages for the simulation, role requirements for each type of actor (terrorist, hostage, negotiator, rescue force member, media representative, simulation organizer), physical setting, equipment needs, and evaluation techniques. Generalizations regarding the running of simulations, as well as the conduct of real-life crisis situations, based on simulation results and empirical studies, are interwoven throughout this discussion.

As with all gaming efforts, *Simulating Terrorism* must ultimately be judged by not only its literary merits and methodological rigor, but also by how accurately it mirrors the real world, and whether its inherent flaws prevent it from achieving its purposes. It is in these two latter areas that Sloan's work, as well as political games in general, will become controversial. Gamers, whether from the Pentagon, the RAND Corporation, MIT, or Model United Nations programs (to mention only a few of the most prominent political science simulations) typically present reams of testimony from their most enthusiastic participants that the experience "felt just like the real thing," and that the problems and emotions evinced by the simulation were accurate reflections of reality. Sloan goes one step further in validation, however, by using entire police units as response forces and negotiation teams, journalists in media roles, and professional actors with previous experience in unconventional warfare as the terrorists.

As might be expected, the utility of the simulations depends upon the participants' abilities, dedication and suspension of disbelief, and the specific operational circumstances of the game. Sloan's games are strongest as training devices in discovering glitches in the standard operating procedures of police forces, such as poor communication (human and equipment failures), conflicting lines of authority, over-reliance on procrustean checklists, and poor preparation of tactical and negotiation teams. The simulations also are a useful means of sensitizing various actors to the problems experienced by those playing other roles, as well as dramatizing some of the moral and operational questions they may have to face in real crises. Police-media cooperation has been significantly enhanced by participation in such simulations, and mock hostages have expressed empathy with the fear of the unknown suffered by real-life victims.

The simulations are somewhat weaker, however, in drawing conclusions about the efficacy of specific negotiation strategies, which detracts slightly from the book's contribution to political science per se. Too little attention is paid—or, at least, reported in *Simulating Terrorism*—to the voluminous literature on bargaining and game theory. Similarly, although Sloan has been a leader in the quantitative study of international terrorism, the book fails to discuss precisely how his simulation results have compared to his statistical studies. The drawing of such parallels would go far to establish the academic respectability of Sloan's work, and perhaps generate additional disciples.

But these are secondary concerns. While other books on terrorism will attract more notoriety, Sloan's book may be the most important contribution in combating one particularly vicious manifestation of terrorism.

EDWARD MICKOLUS

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Approaches to Democracy: Philosophy of Government at the Close of the Twentieth Century. By W. J. Stankiewicz. (New York: St. Martin's Press, 1980. Pp. xi + 276. $25.00.)

Approaches to Democracy is part of a "longer work . . . planned under the general heading of Relativism in Politics" (p. xi). In this, the second volume, W. J. Stankiewicz concerns himself with the effects of relativism on democratic theory and practice—effects which he regards as entirely pernicious.

Relativism, according to Stankiewicz, is the view "that there is no method of reaching agreement about the 'truth' of ethical propositions" (p. 229). He considers this view to be both false and dangerous; rather than demonstrate its falsity, however, he sets out to expose its dangers. These follow from the connection between relativism and egalitarianism, for "relativism can be regarded as the ethics of egalitarianism" (p. 14). If there is no way to establish the truth of ethical propositions, that is, then one person's opinion about what ought to be done is as (un)worthy as any other's, which means that we are all equal in the sense that there are no authorities in morals and politics. Once we acknowledge this, we must regard democratic government as government based on the *preferences* of the governed. This leads, in turn, to an emphasis on procedures for expressing, aggregating, and deriving decisions from these preferences—for example, universal suffrage and majority rule.

The problem, as the author sees it, is that the egalitarian ethics of relativism (or the relativist ethics of egalitarianism?) fail to provide a foundation for democratic government. For if there is no reason to believe one person's convictions are better or worse than another's, then why should the minority accede to the preferences of the majority? Why should anyone even accept the dictates of those in power as authoritative rules rather than mere commands?

This criticism is interesting enough, especially at a time of anxiety about the "crisis of confidence" which seems to imperil ostensibly democratic governments. But this, unfortunately, is about all that one can say in favor of Stankiewicz's book.

Approaches to Democracy suffers from three major defects. First, the book is little more than a loose collection of brief essays, each critical of others who have written about democracy. Chapter 3, for example, begins with a page and a half on "The Problem of Unconscious Relativism in Political Theory," then proceeds with six pages on Schumpeter, six on Niebuhr, five on Macpherson, and a final six on Braybrooke. Then on to the next chapter where a different set of writers is treated in the same manner. And so on, with no discernible direction, throughout the book.

A second defect is Stankiewicz's tendency to misread those he subjects to criticism. Among those he misunderstands and misrepresents are Hannah Arendt, C. B. Macpherson, and John Plamenatz. The misreading of Plamenatz is particularly noteworthy. Stankiewicz quotes the following from *Democracy and Illusion:* "Democracy is a matter of rights and obligations and of procedures that secure rights and ensure that obligations are, or can be, fulfilled. Whoever seeks to explain it, no matter how much his purpose is merely to explain and not also to justify, cannot avoid a kind of exercise which is more properly called philosophy than science; especially if the natural sciences are taken to be models of what science should be" (pp. 195-96). As interpreted by Stankiewicz, this means that "Plamenatz wants to talk about 'rights and obligations and procedures' precisely because the normative element which makes them significant can then be ignored: they can be treated as facts about the State and be analyzed without reference to the norms. Such an approach can make political theory superficially resemble the physical sciences or rather what physical scientists pretend to be doing—namely dealing with the facts" (p. 199). Thus Plamenatz is convicted of a "half-formulated preference for scientism" (pp. 199-200).

Finally, *Approaches to Democracy* fails to present anything more than an ill-defined and ultimately mysterious alternative to relativism. If we are to avoid the dangers of relativism, we are told, we must return to a democratic theory based on "norms." This theory "makes the 'common-sense' assumption that there are certain principles of behaviour—norms—of whose ultimate origin and nature we are not sure, but which in a way provide some general descriptions of behaviour" (p. 252). Moreover, these norms are not simply "subjective judgments"; they are "principles for making choices" (p. 253). But what are these principles? How are we to find or comprehend them? These questions Stankiewicz neither answers nor attempts to answer.

What we are left with, then, is a book which may actually comfort relativists. For despite all the paper and ink Stankiewicz devotes to scoring relativist egalitarians (or egalitarian relativists?), his failure to set out a coherent alternative may leave them more convinced than ever of the correctness of their position.

RICHARD DAGGER

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Marxism and Individualism. By D. F. B. Tucker. (New York: St. Martin's Press, 1980. Pp. 255. $27.50.)

This book delivers both more and less than its title promises. Part 1, "Marxism Reconsidered," does not offer a comprehensive reinterpretation of Marx. Rather it is confined to elucidation of Marx's views on individuality and democracy. Part 2, "Radical Individualism," is nothing less than a confrontation and appraisal of many contemporary theorists, including Barry, Berlin, Dahl, Dworkin, Hart, Macpherson, Nozick, and Rawls. On the basis of this, Tucker develops and presents his own outlook, which he chooses to call "radical individualism." He says "a radical individualist approach to political issues is the most reasonable" (p. 6). Many will find, as I do, Tucker's appropriation of this label somewhat misleading, especially those who rightly associate "radical individualism" with thinkers such as Hobbes, Buchanan and Tullock, Hayek, or Nozick. The position that Tucker undertakes to defend is better understood as a version of "radical egalitarianism" or "democratic collectivism." The case he presents is perhaps the best that can be made and is well worth attending to, especially by those who will find it ultimately unpersuasive.

In part 1, Tucker argues that Marx is best interpreted as both a "methodological" and an "ethical" individualist. This will certainly come as a surprise to those who find merit in the interpretations of Avineri and Kolakowski, for whom Marx is essentially a collectivist, the supreme advocate of social unity. No doubt, as Tucker contends, Marx did affirm the value of human freedom and autonomy. But surely what is distinctive about Marx's reading of the human condition is his anarchistic belief that people could be both autonomous and much more thoroughly communitarian than historical experience would demonstrate or even suggest. Tucker's acceptance of Marxian moral and social psychology appears to be founded on nothing more substantial than conviction, hope, or faith.

The hero of part 2 is C. B. Macpherson. According to Tucker, John Rawls's willingness to entertain a just form of capitalism is "correctly challenged by Macpherson" (p. 123). Tucker says that liberals mistakenly accept "the deeply pessimistic view that modern industrial societies must inevitably embody inequalities of wealth and status if they are to function at all adequately" (p. 99). He endorses Macpherson's assumption that "human beings are naturally industrious" (p. 100). Today people work because they are coerced, but coercion could be replaced by "enthusiasm generated by self-determination" (p. 100). Moreover, liberals, for example Robert Nozick, tend to assume that people are markedly different in innate capacities. But "enough people are capable of being trained as scientists, doctors and lawyers (not to mention managers and directors) to make nonsense of Nozick's assumptions" (p. 174). In any event, "as long as the economic system is based on an incentive requirement which is unjust, it will not be possible to correct for anomalies without precipitating a crisis" (pp. 196-97). Hence capitalism is doomed, and so also market socialism, since it too relies on human competitiveness, not willing cooperation that Rawls would elicit through justice and Macpherson by getting rid of the market.

That Tucker finds persuasive Macpherson's vision of participatory democracy and a command economy does not mean that he is uncritical of Macpherson. Those who are inspired by Marx can learn something from the liberals. It will not do to be neglectful of rights and justice, as Tucker thinks Macpherson is. Indeed, in a genuinely socialist society protection of rights is crucial (p. 125), and a conception of distributive justice is even more urgent, given the absence of the market and Benthamite incentives. Moreover, Tucker does not share Macpherson's confidence that abundance is at hand, and this rather gloomy and un-Marxian prospect renders imperative a coherent and public conception of justice, along the lines already set forth by Rawls. In this connection, Tucker remarks with approval on the thinking of Rawls and Dworkin in which "liberty as an ideal is subordinated to some principle of equity" (p. 138). Somehow a common sense of fairness is to organize and drive the "radically individualist" and socialist society faced with scarcity.

On just how Macpherson's vision, as both diminished and improved, is to be institutionalized Tucker does not have much to say. He does, however, advance a "radical" conception of law, and he offers reflections on "civil disobedience, although discourse on political economy would have been rather more in order. In these chapters Tucker embraces a natural law perspective in which substantive override procedural considerations of justice, as is to be expected when equality takes priority over liberty. In this perspective, civility is "primarily a capacity to think and act in a principled way" (p. 221). But evidently in certain circumstances "terrorism" is not forbidden if it is the best way to achieve democratic political goals (p. 222). A disturbing note in an otherwise civilized book.

A number of questions arise about the case for "democratic collectivism" as presented by Tucker. The first have to do with our human nature. Are we really so constituted as to make the vision workable? We may well be both more ambivalent and pluralistic, both more competitive and ambitious, and more unequally endowed in both ability and ambition, than he thinks we are. These are empirical questions, as is the question whether we could undergo a transformation in which both social and economic competitiveness are eradicated and replaced by the postulated comunitarian autonomy. Surely it is high time to leave the realm of assertion about these matters.

Moreover, something feels incoherent about a society in which "civility," understood as expressivist freedom, is to be combined with some kind of unspecified rational, centralized, and democratic planning. Tucker says, "for reasons I have never found convincing, liberal theorists are reluctant to count property claims . . . as constituting a violation of the freedom of others" (p. 115). Perhaps people who feel and think this way would be more at home in a market than in a planned society.

Finally, Tucker and democratic collectivists generally seem rather cavalier about the implications of getting rid of the market economy. It would mean above all the thoroughgoing politicization of life. And suppose all the human energy and ambition that now go into economic endeavor were to flow into political competition, what an Hobbesian life it would be!

JOHN W. CHAPMAN

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Trials on Trial: The Pure Theory of Legal Procedure. By Gordon Tullock. (New York: Columbia University Press, 1980. Pp. xiii + 255. $20.00.)

This book is a provocative if somewhat randomly organized challenge to some of the procedural rules and roles of American courts. Economic tools of analysis are its stock in trade, reduction or control of the costs of legal procedures its pervasive theme. Gordon Tullock is less interested in the "fairness" of legal procedures than in their accuracy and costs. He does not suggest that these are the exclusive goals of a legal system, only that the critical perspective of an economic calculus is likely to yield more *socially* just results, e.g., verdicts which accurately and efficiently apportion legal responsibility for particular acts. Who can quarrel with such a goal? Yet it is disconcerting to confront an analysis of our courts which so casually sets aside the norm of perceived (and perhaps real) individual fairness. The book must be read on two levels: it is an economic interpretation which challenges some settled ways of conducting legal business, but it is also a very sophisticated political statement.

The book is least political and most challenging when it deals with the risks and costs of litigation strategies, and when it examines the meaning and impact of rules of evidence (such as the ban on hearsay) and decision rules such as "beyond a reasonable doubt" or "fair preponderance of the evidence." In a criminal case, for example, the government must prove "beyond a reasonable doubt" that the defendant is guilty. But what is "beyond," and what is a "reasonable doubt"? In all likelihood, these terms mean different things to different people; is it 90 percent certainty? 80 percent? Assuming that 85 percent is the modal probability for average jurors (e.g., they will vote to convict when they are 85 percent certain a defendant is guilty), a certain number of improper convictions *and* improper acquittals can be expected. The standard "beyond a reasonable doubt" establishes a bias in favor of the defendant, which generally means that, relatively speaking, there will be fewer improper convictions and more improper acquittals. If the standard of proof were lowered to the civil case standard of "fair preponderance of the evidence," then presumably there would be *fewer* improper acquittals and *more* improper convictions. Exactly how much these ratios would change is an empirical question on which there is, at present, no evidence. The probability of conviction affects deterrence; but so too does the severity of the penalty and the certainty of conviction and punishment. Tullock's partial analysis offers very useful insight into the probable consequences of manipulating those decision rules.

On the question of government subsidies of legal costs, Tullock's analysis has more obvious political ramifications. In the "ordinary" private lawsuit, each side pays its own costs. The taxpayers bear the cost of the forum and ordinarily have no interest in the outcome except that it be reached impartially. But increasingly since the 1960s, government has undertaken to underwrite the legal costs of indigents, either in suits against the government itself or against other private persons (some of whom may themselves be indigent). In criminal cases, government pays all the costs of prosecution and since a very substantial majority of defendants are indigent, the costs of legal defense as well.

Tullock argues that the purpose of subsidizing private legal fees is to increase the likelihood that the proceedings will result in a just conclusion. But it is not at all obvious, he says, that this policy will produce the intended results, especially where government is subsidizing *both* sides of a case. How can one calculate the "optimal" amount of resources to be invested in a case, optimal being defined loosely as the most just outcome for the least cost? In a hypothetical divorce case involving a custody dispute over one child where both husband and wife are indigent, should government pay the costs for both the husband and wife? Should each receive equal resources? No, says Tullock, "unless we just have some primitive ethical idea involved here." Instead, assume that the wife has an 85 percent chance of obtaining custody of the child, the husband 15 percent, and assume further that the government decides to invest $500 in representing the wife and $750 in representing the husband. This would be an uneconomical way of assuring that the husband obtained his 15 percent chance and the wife her 85 percent chance. Instead, if we assumed that if neither side were subsidized, the husband would have a 10 percent chance to obtain custody and that a small subsidy of $25 would increase that to 15 percent, government could save $1,225 and still assume a high probability of the "desired" outcome.

The estimates are arbitrary, of course; one can easily quibble with them. But it is the fundamental assumptions and their likely consequences which are disturbing. Tullock can find no convincing reason why "we might prefer to have the government paying attorneys on both sides to having them get the same probability of outcome by helping only one side" (p. 117). He rejects any value that might come from using an adversary procedure (which implies equal resources on both sides) for its own sake. He does not believe that we are substantially increasing the potential accuracy of court procedures by increasing resources on both sides: "Doubling the resources of both the man who represents the truth, and the man who represents falsehood" (p. 118) may increase the probability of arriving at the truth, but only slightly. Thus in the hypothetical divorce case described above, if neither husband nor wife were represented, the odds favoring the wife (given current child custody award practices) are about nine to one. If both sides are represented, then, at most, the husband's chances increase to 15 percent. Is this slight increase worth the government's subsidy? Tullock, clearly, believes that it is not.

The implications of this view, in the context of the proposed extinction of the Legal Services Corporation, are obvious. Most government subsidies for legal representation of the indigent are not worth the cost. If the reasoning is applied to criminal cases, the consequences are even more pernicious. If the defendant in a criminal case faces a 95 percent chance of conviction, and if you assume, as Tullock does, that the prosecution has reasonably accurately screened out those cases in which guilt is at least doubtful, then what is the government's interest in subsidizing the costs of a legal defense? If one assigns no value to legal representation itself, and if one assumes that government should be willing to tolerate increased numbers of wrongful convictions because that will reduce the number of improper acquittals (e.g., fewer guilty persons will go free, but more innocent persons will be acquitted), then the "logic" of the solution is obvious.

JOEL B. GROSSMAN

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American Politics

We Shall Not Overcome. By Robert Emil Botsch. (Chapel Hill: University of North Carolina Press, 1981. Pp. xv + 237. $19.50.)

Is the South ready for a populist revival? That is the question addressed by this in-depth study of the attitudes of ten white and five black males in a North Carolina city dominated by furniture industries specializing in non-unionism and low wages. Many students of southern politics have expected a return to a politics of the haves versus the have-nots with the passing of the race issue in the region's politics. Using the technique developed by Robet Lane in *Political Ideology* (Free Press, 1962), Robert Botsch explores that possibility.

This study does not support the likelihood of a future populist movement. Botsch finds the economic misery on which to build such a movement and a recognition of class distinctions; yet his interviewees have a strong sense of individualism, and like Robert Lane's workingmen, they believe that status should be and is achieved by merit. As with Lane's workingmen, religion is a conservative force in the interviewees' acceptance of status. Furthermore, racial beliefs continue to be a factor in separating these workers. While many readers may find the racial views of these whites quite moderate in comparison to what might have been expected, these men still think in terms of stereotypes and prejudices that could undermine a populist movement. In particular, the whites look with significant suspicion on affirmative action efforts.

Botsch writes that the greatest obstacle to populist success is the "fatalistic, individually adjusting, and coping stance that these men take toward most of life's situations" (p. 199). A significant consequence of this attitude is the lack of political participation by the respondents. Only five of the fifteen voted in the 1976 presidential election, though that contest featured a southern candidate. Botsch also argues that these men fail to vote because the system makes it too difficult for them to register and vote. However, even if they were registered to vote, their failure to view politics as a mechanism to right the wrongs of society suggests that an organized, class-based politics is not likely in the South.

The reader may be somewhat suspicious of Botsch's sample and findings. The in-depth interview worked very successfully for Robert Lane, and most political scientists would support its usefulness in understanding the thinking of a particular group. However, some readers will certainly wonder if these fifteen southerners are typical of the blue-collar community. For example, five of the fifteen could not afford to own an automobile. None was a woman. All were under 35 years of age. While no typical southern blue-collar worker may exist, the representativeness of this group is suspect.

Though the sample raises questions about the results, the author's orientation to the study reduces further its effectiveness. Apparently, Botsch wants to find the makings of a populist movement. While he is to be commended for his frankness, the academician may find such partisan overtones counterproductive. Even so, Botsch's findings are congruent with my perspective as a participant-observer of southern politics.

Regardless of how the readers judge the merits of Botsch's conclusions, most will agree that too much attention is paid to quotations from the interviewees. The author tells the views of each respondent on many questions; as a result, generalization and analysis is thin compared to the emphasis on individuals.

Finally, readers may wonder if Botsch has failed to capture an increased emphasis on economics in southern politics with a corresponding decline in the significance of the race issue. Successful candidates in southern politics often portray themselves as the workingman's candidate. However, such portrayals do not often extend to support of economic liberalism. The work of Botsch makes clear why it is not necessary for candidates to do so in order to gain the support of working-class southerners. In doing this, he makes a valuable contribution to the literature on southern politics.

GLENN ABNEY

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The Power of Money: A Political-Economic Analysis with Special Emphasis on the American Political System. By Henry L. Bretton. (Albany: State University of New York Press, 1980. Pp. xxxiii + 418. $34.00, cloth; $12.95, paper.)

Money, like sex, preoccupies and fascinates a lot of people. Indeed, in this era of economic crisis and jaded erotica, some individuals are finding money to be the sexier subject of the two. So they have money desires, money anxieties and even money fantasies. What they—and we—don't have, Henry Bretton tells us repeatedly in this book, is a proper grasp of the importance of money as a political force and the dangers it poses to democracy. According to Bretton, and contrary to what monetarists and most other economists have claimed, money is anything but a purely neutral instrument of exchange. Money gives people new and far-reaching means of controlling resources, exercising power and shaping social relations. The moneyed influence is evermore concentrated, impersonal, mobile, anonymous and unaccountable.

To make matters worse, observes Bretton, the political leaders of modern governments have but a feeble grasp of what Bretton likes to call "matters monetary." (I once read that John F. Kennedy, one of our better-informed and more literate presidents, had to rely on a rather simple-minded mnemonic device to help him remember the difference between "fiscal" and "monetary," thereby revealing his rather simple-minded understanding of such matters.)

The moneyed interests, Bretton continues, circumvent the intentions of lawmakers, encompass the public weal, and bend things to their own will. The monetary system operates according to its own imperfectly understood dynamics. Not just its clumsier moments of corruption, but also the "system as a whole" can be viewed as pathological because it has become so "monetarized" and has fallen so thoroughly under the influence of forces inimical to democratic political processes. "Advocates of structural or procedural change as an antidote to bribery and corruption, miss a basic point. Corruption is no longer a question of yes or no, but of degree" (p. 333).

What the monetarized society reveals—if we are willing to look—is the incompatibility between capitalism and substantive democracy. We may retain the trappings of democracy, but the social and economic costs of big money are more than the polity can bear, producing ever-greater wealth and power for the few and more scarcity and hardship for the many—a process which, the non-Marxist, anticommunist Bretton notes, was best elucidated by Marx and not caused by him. It is time we retire the notion, he concludes, that we can comprehend and meaningfully analyze such concepts as nation, polity, society or international society, without reference to the centrality of money.

This book's message is an urgent one for political scientists and economists alike. Yet it is seriously flawed and sometimes a chore to read. For one thing, the entire first half of *The Power of Money* is dedicated to criticizing (in an often repetitive and not very penetrating way) the economists, political theorists and other social thinkers who over the centuries have failed to see the link between money and political power. Bretton ponders whether their myopia has been due to their ideological perspective or their material self-interest, as if these two influences must be treated as mutually exclusive. Commenting on the inability of modern political scientists to see the power that money has over the political order, Bretton observes that this grand evasion "should not be surprising for the risks faced by the violator of the professional code" have been great, involving possible loss of academic credentials and loss of public and private funding. Money doesn't go to "troublemakers" (p. 117). But he does not explain how the professional code came to be, who enforces it and why a failure to ignore certain subjects brands one a troublemaker in academia—as indeed it does.

And while Bretton marvels at how economists can overlook the political dimensions of money and political scientists can avoid the monetary aspects of politics, we, in turn, might wonder how he can write a whole book about the power of money and say almost nothing about the expropriation of labor, surplus value, capital accumulation and class conflict—all of which compose the very context of the monetary system. Actually it is not always clear what Bretton means by money: it is something more than actual currency, and something other than property, wealth or capital. He says money must be understood in the context of a whole social order, but he has almost nothing to say about capitalism as a social order.

The reader will also yearn for some analysis of the modern monetary system itself, one that would be helpful to the political scientists and economists who want to connect the subject of power and money in just the way Bretton repeatedly urges them to. Bretton keeps *telling* us that the monetary system has brought a momentous visitation, transforming social relations, political power and even human nature itself, but he doesn't *show* us what it is about the monetary system that does this. He doesn't get into the concretia, the imperatives of the monetary process or some key aspect of it. He never offers the X-ray picture, a few well-chosen cases showing *how* money generates *economic* power in a way that is peculiar to money. Instead we have to take his word for it. For instance, he tells us that an increase in the quantity of money in circulation has significant socioeconomic repercussions, affecting people differently depending on their money position, and on another occasion he observes that holding the note is better than owning the assets. These are crucial propositions and I think they are true, but how so? why so? We'll never find out in this book.

The latter half of *The Power of Money* gives us a detailed account of how money is used to buy influence in the American political process, a treatment that should be familiar and useful to any student of lobbying, elections, and interest-group politics. But what we do not get is any exposition of the unique and historically significant way that money creates economic and social power, and what systemic imperatives are generated by the monetarization of society's productive forces.

In sum, this study gives us too much review and overview of other writers' shortcomings on the subject and not enough of the author's own analysis of the real-life phenomenon he says is so important to analyze. I have always disliked the fashion of applying sports terms to political things, but I must say, Bretton's pitching suffers from too much wind-up and not enough delivery.

MICHAEL PARENTI

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Intellectuals and Other Traitors. By Bernard E. Brown. (New York: Ark House Ltd., 1980. Pp. xi + 196. $12.95.)

A sizeable minority of the intellectual class has withdrawn from the democratic consensus, according to Bernard Brown. They are traitorous because they attack and weaken the democratic polities that offer them autonomy and independence. All of them, without exception, seek overthrow of what they conceive to be the capitalist system along with its social-democratic versions.

All are ruthless in their opposition to fascism; they identify fascism with the ultimate stage of capitalism and monopolistic development.

Who are these traitorous intellectuals? They are to be found among university faculty, journalists, research scientists, novelists, poets, artists, managers, and lawyers. Their philosophy is anticapitalist, Marxist, and some are even anarchist. They first became a significant force during the 1960s, and they can still be found in the universities of Western Europe and the United States. These traitorous intellectuals are alienated from modern society as they understand it. Why are they alienated? Some are anarchists who are critical of almost everything that exists. Others are Marxists who admire Marxist heroes such as Fidel Castro and Che Guevara. Although these intellectuals lack a highly unified organization, they have established a power base within our universities. The unifying theme that brought them together in the 1960s was opposition to the American role in Vietnam.

Brown believes that universities are "indicators" of intellectual currents that run fast and deep in any society. They are a distant mirror of government, and they enable us to read a society's vital signs. Because our universities are now adrift in a climate of dissensus, demoralization, and lack of discpline, they will soon adversely affect our society and our political system. Revolutionary intellectuals are especially dangerous because they have taken positions of power within universities from which they will be able to extend their ideas.

Intellectuals in the United States are experiencing poverty—not the financial kind as some of us might think—but rather a poverty of perception and understanding. For example, they fail to see that the appalling conditions of the poorer classes in our cities do not arise out of the evils of capitalism, or other causes such as prejudice, slavery and unemployment. Rather, the deprivation of the poor is a result of their abysmal lack of discipline. Children of the hard-core poor lack the motivation they need to acquire the knowledge and skills that will enable them to function in a modern economy. Social institutions such as family, church and schools no longer provide the kind of discipline needed by these young people. Unemployment, which is most serious in the cities, is not a cause but a consequence of this lack of discipline. Masses of Puerto Ricans and Chicanos in the United States, and Third World immigrants in Europe are like a powder keg. This explosive mixture may be ignited at any time by intellectuals who are under the spell of Marxist dogma. Critical social scientists should be blamed for distorting and misinterpreting our social problems. If allowed to continue, the pretentious arrogance of revolutionary intellectuals will lead straight to an ideology of authoritarianism and totalitarianism.

What we need to do, according to Brown, is to recreate and invigorate the structures of discipline, and we should begin with our educational institutions. We need curriculum reform, higher admission standards for students, and tougher professional criteria for teachers. Institutions of higher learning have become corrupt and degraded. It would be better to close them down altogether rather than allow them to decline further. Too many students specialize in legal and vocational training. The study of humanities and social sciences needs to be revived. Ambitious young people should have outlets for their energy and talent outside of law schools and business schools.

At the international level the United States must take a tougher stance. It must reconsider aid to Third World countries; its policy of acquiescent cooperation with the United Nations must be reversed. Russia, too is a growing menace; a tougher policy will have to be taken toward that superpower. At the national level the problem is big government. Too much political power has been vested in the judges and armies of lawyers who are being turned out by professional schools.

Bernard Brown's book is advertised as one that should be read by presidents, prime ministers, governors, mayors, congressmen, aspiring politicians, diplomats, and other citizens. Readers of this review, having read the preceding summary, can judge for themselves whether this publication merits that kind of an audience. Perhaps most academics will suspect that the label "traitor" might be used against the author. At a time when the "poverty" of many intellectuals consists of low income, Brown has turned against his colleagues with McCarthy-like insinuations. In one paragraph, he disclaims any intention of pillorying intellectuals in general, and then he makes sweeping allegations throughout the book that can be used by the political right to undermine our educational institutions. Colleges and universities remain the only institutions where the almost total hegemony of business values and the profit motive may be challenged. Criticism such as the author's can be used to further weaken these increasingly vulnerable centers of social criticism. Readers who look for an antidote to the one-sided arguments in this book can find it in *Confessions of a Conservative* by Gary Wills.

MERLIN GUSTAFSON

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In Struggle: SNCC and the Black Awakening of the 1960s. By Clayborne Carson. (Cambridge, Mass.: Harvard University Press, 1981. Pp. 359. $22.00.)

This splendid history of the Student Nonviolent Coordinating Committee (SNCC) has successfully captured the dynamic interplay of two parallel but contradictory elements. On the one hand, SNCC represented a mixture of idealism, spontaneity and sacrifice on the part of black and white student activists. SNCC field workers saw themselves as a family, a "band of brothers," dedicated to helping blacks fight for their American rights and to challenging frontally white supremacy and oppression. SNCC workers were also consumed, however, by a burning sense of frustration and a radical disillusionment with both black and white allies as well as with American society. They resorted increasingly to revolutionary rhetoric and an attack against imperialism and capitalism. Eventually, a corrosion of distrust and suspicion helped destroy the "band of brothers." Symbolic of the 1960s—with its heady excitement of student activism, hope, rage and violence—SNCC never really survived that decade.

SNCC's successes were achieved in the rural Deep South. Discrimination there was pervasive and blatant, susceptible to a variety of strategies and tactics, and exploitable on the campuses of the black colleges that dotted the landscape. Originally composed of representatives from student protest groups, SNCC was quickly taken over by its own field workers who became the SNCC. Their initial reliance on direct action—demonstrations, sit-ins and freedom rides—reflected both the original student sit-ins that led up to SNCC and the ideological commitment of the early activists to Christian radicalism, nonviolence, and a belief that the immorality of segregation could be exposed and destroyed by the peaceful, moral behavior of the demonstrators.

A competing strategy, which eventually predominated, stressed the importance of voter registration so that black southerners could begin to control the political system. Both strategies relied upon articles of faith: that the Kennedy and Johnson administrations could be induced to intervene massively on the side of civil rights, a faith not immediately realized, and that the social-political system was worth changing and could be reformed.

Voter registration drives led to the establishment of education and other types of projects in southern black communities. SNCC's approach focused on training local leaders, building community-based organizations, and fighting for locally defined issues. A Black Panther party was organized in Lowndes County, Alabama, and SNCC played a major role in helping create the Mississippi Freedom Democratic party. During this period, white workers and volunteers were active and welcomed.

When it refocused its efforts on urban blacks and the North, SNCC was never able to find a successful combination of ideology and strategy. A different group of activists had taken over SNCC, some with no experience in the South and with black nationalist backgrounds. As a result of its emphasis upon urban and northern blacks, almost all SNCC projects in the rural Deep South were allowed to wither. The articulation of Black Power that started as a tactic for arousing the enthusiasm of the black masses soon became both an ideology and a strategy into itself. Yet SNCC leaders were never able to build organizations or programs on the basis of this slogan. At the same time, a surge of racialism in SNCC led to the exclusion of white workers and volunteers. The Black Power slogan, the rhetoric of violence that often seemed to accompany it, and the emphasis on racialism alienated most northern supporters as well as other black organizations and eventually led to violent police repression and FBI infiltration.

Political scientists will be particularly interested in SNCC's relations with the Black Panther party and the Mississippi Freedom Democratic party as well as with the peculiar organizational nature of SNCC, with its anti-leadership, anti-unity and anti-discipline orientation. The constant search for an ideology and strategy to keep SNCC relevant in the black communities and to assure leadership for SNCC as the most radical of black civil rights organizations also merits attention, as does SNCC's ambivalent relationship with other black civil rights groups. This is a well-researched, balanced, and analytical assessment of the history of a primarily black student activist group that, with all its failings, made its special contribution to the political awakening of American blacks and to the changing of American institution and practices.

ABRAHAM HOLTZMAN

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Kennedy and the Berlin Wall Crisis: A Case Study in U.S. Decision Making. By Honoré M. Catudal. (Berlin: Berlin Verlag, 1980. Pp. 358. DM 36, paper.)

This volume, first of two projected by Honoré Catudal on the Berlin Wall, recounts the expectations and actions of the U.S. and other governments from early 1961 through the erection of the first barbed wire fences in August of that year. Catudal has written other monographs on divided Germany, and builds his narrative from meticulous examination of the literature—articles, books, documents, materials obtained under the Freedom of Information Act, and JFK Library archives. He has also interviewed and corresponds with many U.S. officials.

His conclusion: the Americans and their allies expected the East Germans to do something to stay the emigration hemorrnage to the West; the West was not caught unawares; President Kennedy rose above the bureaucracies to determine U.S. policy—vindicating the "rational actor model" of decision making, at least in this instance.

While Catudal's narrative is most engaging, his analysis is flat if not weak. Much of his evidence undercuts his own conclusions. Thus the Deputy Director, Office of German Affairs, State Department, admitted "it was very difficult to conceive before the Wall went up that the East German regime . . . would make such a blatant confession of inability to compete" (p. 29). And if the president was able to impose his will, this was partly because startled State Department duty officers could find no contingency plans when they learned of the wall. Even if the West expected the East to do something, the latter's timing and specific actions came as a surprise.

Although Catudal believes the president prevailed, the State Department's Berlin Task Force Duty Officer complained (p. 52) that the "entire operation was at times a bit rudderless." One comes away thinking that Graham Allison's modes of explaining decisions should be expanded by at least one: individual and collective bungling (lost papers, time consumed in interagency clearances, lack of secure phones, etc.).

Catudal concludes (p. 253) that "rationality (however construed) may indeed play a crucial role . . . [in] foreign policy. In fact, Kennedy's controversial decision not to challenge Khruschev . . . represents one of the most clearly documented examples . . . of a President rising above 'bureaucratic inertia' to play an active . . . role." To offer this proposition as a clarification of decision-making theory is unwarranted. First, "rationality" cannot be construed just any way; it must be tightly defined. Second, a decision "not to challenge" is to *follow* inertia—not to rise above anything. Third, Catudal seems not to understand that "rational-actor decision making" posits that the government act as a *unified team* to maximize gains and minimize losses given its values and available information. Though Washington and its allies wound up sitting on their hands, Catudal shows that there was considerable bickering *within* the U.S. government and between Washington and Europe in the months before August, helping to paralyze the West when the first wire was unrolled.

A foreword by a participant (Martin J. Hillenbrand) praising the book for giving his team a provisional clearance is hardly persuasive.

Catudal's chronology, though detailed, misses major contextual events—McNamara's missile program unveiled early in 1961 and Moscow's resumption of nuclear tests in August.

The forest is missed for too many small trees in this apologia for one of Kennedy's not-so-fine moments.

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Politics and the Constitution in the History of the United States: Vol. 3, **The Political Background of the Federal Convention.** By William Winslow Crosskey and William Jeffrey, Jr. (Chicago: University of Chicago Press, 1981. Pp. xii + 592. $27.00.)

William Winslow Crosskey (1894-1968) published in 1953 the first two volumes of his study on "politics and the Constitution." They covered the Constitution from the time it left the Philadelphia Convention, with emphasis on the early years of its interpretation (or misinterpretation). We now have an account, prepared by Crosskey and completed by William Jeffrey, Jr., of events leading up to the convention. A final volume, for which Crosskey left notes, is planned on the convention itself.

Crosskey's work is neither narrative history in the tradition of Charles Warren nor commentary in the tradition of E. S. Corwin. It is rather a work of deliberate constitutional ideology in a historical mode, like that of Louis Boudin. The curious epigraph to the current volume sets the tone. It is the explanation by Patrick Henry, after the Constitution had been drafted, why he had declined to serve as a member of the Virginia delegation: "I smelt a Rat." Crosskey's study is presumably an attempt to sniff out the truth of this statement.

The Constitution, Crosskey holds, is a perfect document; that is, it describes a logically complete political system. Once its vocabulary is understood, for which Crosskey compiles a "dictionary of 1787," and once eighteenth-century canons of legal construction are employed, the document becomes clear and unmistakable in its meaning.

Applying these assumptions and rules to the political discussions of the 1780s, Crosskey concludes that the Constitution establishes for Congress a plenary control over the nation's economy, permitting no distinction between interstate and intrastate commerce, and reserving to the states nothing that touches upon commerce or upon any of the objects of the preamble. As Crosskey states it, the Constitution "establishes no such fantasy as 'the delicate balance of our federal system' " (p. 22). Rather, the framers established a "complete national power of regulating commerce" (p. 458).

When Crosskey's first volumes were published, law journals devoted to them more than 400 pages of mostly unfavorable reviews, an honor probably unique in American legal history. Political scientists and historians took the work less seriously, and for them, *Politics and the Constitution* soon vanished. The reasons for this disappearance are, briefly, that Crosskey's political aims had largely been achieved with the Constitutional Revolution of 1937 and that, while his historical knowledge was prodigious, his views of history, to say it plainly, were strange.

These traits continue. The present volume is a chronological account of those events which from 1775 to the eve of the federal convention contribute to establishing Congress's power over commerce. The story, with its lengthy and often fascinating excursions into private correspondence, is rigorously organized and necessarily selective. It contains notable discussions of efforts by New England merchants in 1785 on behalf of a national commerce power; of the effects on the South of a proposed treaty of 1786 which, for a limited period, would have exchanged U.S. navigation on the Mississippi for access to Spanish markets; of the role of Shays' Rebellion in calling the federal convention; and of Madison's purported falsification of proceedings in the Continental Congress for a crucial day in February 1786.

Crosskey's aim in all this is explicitly political. Like its predecessor volumes, this one is dedicated to Congress "in the hope that it may be led to claim and exercise for the common good of the country the powers justly belonging to it under the Constitution." For the present, this is surely a noble futility: we can expect little sympathy from the judiciary (see *National League of Cities* v. *Usery,* 1976) or from the Congress and executive who stand for a concept of federalism that Crosskey loathed.

Crosskey holds a remarkable view of the nature of history. In place of the complexity, uncertainty, compromise, or even luck, which normally make up political history, Crosskey has discovered a constitutional teleology, a single force that moves through American history in the years preceding the federal convention and knocks aside all other issues—slavery, the size of the republic, the method of representation in Congress, a bill of rights, foreign affairs, the federal executive, interests of the separate states. That force, congressional power over the economy, naturally triumphs at the end, despite the efforts of many to thwart it both before and after it was incorporated into the Constitution.

This view of history requires, concomitantly, that we be able to leap across the centuries and accept today precisely what the framers left for us, regardless of intervening Supreme Court interpretations. This means that Crosskey sets out a Constitution that did not gather—or at least should not have gathered—anything in the way of tradition or gloss over the years. It is a mystery how it survived the eras of continental expansion, dual federalism, internal improvements, slavery, the Civil War and Reconstruction, industrial capitalism, laissez-faire, the welfare state, and modern civil liberties.

Apart from his political aims and his odd historical premises, Crosskey's style deserves special notice, for it blocks acceptance of his views. He overstates his claims. He advances his argument through labyrinths of ingenious legal logic, alternately seducing and bullying the reader. He describes in caustic language the states'-rights devils who inhabit his landscape, and their Antichrist, Madison. He candidly, but very liberally adds italics to his thousands of quotations and so invites suspicion of his use of evidence. His 70 pages of footnotes contain no secondary sources, only a few admonitions against Merrill Jensen's 1940 work on the Articles of Confederation.

Crosskey's breadth of concerns and even his obduracy nevertheless lead us to look again at certain problems in constitutional law: a substantive meaning for the preamble, the "no federal common law" doctrine of *Erie Railroad* v. *Tompkins* (1938), new uses for the contracts and ex post facto clauses, and the possibility that Congress, under the commerce clause, could supersede the work of the National Conference of Commissioners on Uniform State Laws. Many of Crosskey's historical interests also deserve further study. Besides the discussions in the book already mentioned, these include the nationalist views of former British Loyalists, of foreign observers with no local attachments, and of state politicians who advocated the opposite courses of wage and price fixing on the one hand and free trade on the other.

Crosskey was a great eccentric in constitutional scholarship. Although he has built a structure from authentic materials, he has handled them in such an idiosyncratic fashion, that even if we are in awe of the result, we are bound to return to more traditional ways.

CHARLES A. MILLER

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The Court Years 1939-1975: The Autobiography of William O. Douglas. By William O. Douglas. (New York: Random House, 1980. Pp. xi + 434. $16.95.)

Independent Journey: The Life of William O. Douglas. By James F. Simon. (New York: Harper & Row, 1980. Pp. viii + 502. $16.95.)

Judicial behavioralists, including this reviewer, have been married by convenience to William O. Douglas. In our scalograms, bloc analyses, and roll-call voting studies, he pleasingly, consistently, and ideologically shows up on the liberal dimension of judicial policy and thus confirms our explanations that the judicial process is, at bottom, just politics and ideology. Both Justice Douglas and his biographer, James F. Simon of New York Law School, joyously endorse that which we have labored so hard to prove empirically.

Douglas did not go to the Supreme Court naive about the judicial process: his law school and government colleagues included such hard-headed legal realists as Thurman Arnold, Jerome Frank, and Fred Rodell. But Douglas professes the influence of a conversation, early in his justiceship, in which Chief Justice Hughes told him: "At the constitutional level where we work, ninety percent of any decision is emotional. The rational part of us supplies the reasons for supporting our predilections" (Douglas, p. 8). Douglas easily embraced this view, for his was an ideological world in which liberal/conservative conflict pervaded every incident, personality, and lawsuit. "Judges," he pronounces, "are not creative figures; they represent ideological schools of thought that are highly competitive" (Douglas, p. 8). And he scorns "the Frankfurter stance that he was following 'the law' and putting aside his own feelings and desires in reaching a decision. Frankfurter's skein of life was woven with a design that was duplicitous, for no one poured his emotions more completely into his decisions, while professing just the opposite" (Douglas, p. 34).

Acknowledging with zest the ideological and political nature of constitutional adjudication, Douglas was oddly irritated by his colleagues' vote-seeking. He marked the "three active proselytizers" during his Court years as Justices Stone, Black, and Frankfurter, with FF earning the sobriquet of "proselytizer extraordinary" who engaged in endless "pleading, needling, nudging, probing," who "never stopped trying to change votes on a case until the decision came down," and who "used his law clerks as flying squadrons against the law clerks of other justices and even against the Justices themselves" (Douglas, pp. 22, 173). Yet Douglas himself was not above mean political maneuver. His threats to publish attacks on Chief Justice Burger—for an attempt to change the Court's rehearing rules in the El Paso Natural Gas Company antitrust case and for making opinion assignments when in the minority in the *Abortion Cases*—were apparently successful. However, Simon reports (p. 250) that in the main Douglas, the realist ideologue, shunned vote-seeking and off-the-bench lobbying, and he quotes Douglas's judgment "that the only soul I had to save was my own."

Douglas's approach invited withdrawal from the Court's collegial process. Although the "conference . . . sometimes changes one's view of a case," he wrote, "the chief function . . . is to discover the consensus" (Douglas, p. 34). Thus, as Simon points out, Douglas rarely asked questions during oral argument, devoting himself instead to scribbling drafts of opinions or of one of his numerous books, and spoke only briefly in conference. And "while others were busily drafting their opinions and submitting them to colleagues for comments (and, they hoped, approval), Douglas wrote exclusively for himself" (Simon, p. 353). Nonetheless, Douglas's infrequent, but pointed, questions to counsel demonstrated that he followed oral argument in detail. And Justice Fortas recalls that while "Douglas would say very little at conference, . . . what he said always went to a critical issue in the case" (Simon, p. 353). Especially nettlesome to colleagues was Douglas's habit of hurrying from the bench to chambers to dictate his views, which anticipated not only how the majority would decide, but also why, and would set forth Douglas's dissenting or concurring reasons.

Douglas's curious self-isolation in the Court stands in sharp contrast to his involvement in extra-curiam political maneuvering. He actively lobbied the Kennedy administration to appoint his friend and ally, Judge James Skelly Wright, to the High Court. He encouraged his friends to promote him for the presidency and vice-presidency. And he remained influential in Washington liberal Democratic circles at least until the Johnson administration.

Simon supplies striking evidence of the extent to which Douglas engaged Frankfurter in a liberal/conservative struggle throughout the legal community. He reports that Douglas arranged to review favorably a book by his ideological ally Edmond Cahn; that Douglas actively supported tenure at Yale for his supporter and former law clerk, Vern Countryman; that he expressed concern at the appointment of former Frankfurter clerks to the Yale faculty; that he was continuously concerned about whether his allies or Frankfurter's were being selected for prestigious lectureships or to supply articles to journals of opinion or law. This mobilization of legal support for conflicting ideological camps on the Court, suggested more than two decades ago by Jack Peltason, has not been systematically studied. We are unsure whether it continues in the absence of two politically savvy, law school-connected, and combative justices such as Frankfurter and Douglas.

His extra-curiam politicking offers a vantage point from which to appraise Douglas's justiceship. A self-identified ideologue, Douglas saw himself connected to the larger liberal movement outside the Court, and his role was to promote that movement. Initially, therefore, he embraced Hugo Black's formulation that the Fourteenth Amendment's due process clause applies all guarantees of the Bill of Rights to the states and Holmes's "clear and present danger" test for the protection of expression. But, unlike Black, Douglas was guided by ideology, rather than by history or judicial precedent. As Black's agenda was accomplished, Douglas felt free to shift his approach to more extended liberal goals by relying on a very general purpose inferred from the constitutional text—namely, that "the Constitution . . . was designed to take the government off the backs of the people." And to ground the creation of new rights, he now "was inclined to agree" with earlier opinions of Wiley Rutledge and Frank Murphy that "the guarantees of due process were not necessarily limited to the provisions of the Bill of Rights but include other privileges and immunities" as well (Douglas, p. 55).

Moreover, to encourage vindication of newly developing rights, Douglas gave narrow scope to rules limiting jurisdiction, such as the political question doctrine. And he sought to expand the classes of persons with standing to bring lawsuits. Douglas would not only authorize all taxpayers to litigate as "private attorneys general," but also in environmental matters, he would allow "those people who have so frequented the place as to know its values" to bring suit on behalf of "valleys, alpine meadows, rivers, lakes, estuaries, beaches, ridges, groves of trees, swampland, or even air" (*Sierra Club* v. *Morton*, 405 U.S. 727, 743).

Douglas's target was not ultimately his judicial collegues, although he expressed great satisfaction with liberal successes during the Warren Court era. Rather, Simon argues, Douglas, the liberal political leader, was appealing to a wider public audience. His opinions are characteristically short, often pungent, and generally address the large issues of interest to attentive citizens. More than 30 books and hundreds of articles, reviews, and speeches were an extension of his opinions. All were intended to persuade citizens at large, in the belief that ultimately the liberal creed would succeed or fail by winning adherents in the democratic political process.

Douglas's activism, his opinions, and his ideology polarized sentiment toward him. Simon points out (p. 252) that "with each libertarian opinion, Douglas's standing among political liberals was enhanced. Among legal scholars, however, his reputation was not so sturdy. Douglas was guilty, some scholars suggested, of inattention to legal detail and indifference to precedent." Indeed, the criticism goes farther—that Douglas betrayed his judicial duties by converting his every political belief into constitutional doctrine. "Result, not theory, was Douglas's judicial litmus test."

In this, Douglas placed his bets on history and on politics rather than on his judicial colleagues or the community of legal scholars. Herbert Wechsler, an advocate of judicial neutrality and reasonableness, observed in his renowned Holmes Lecture that "history has little tolerance for any of those reasonable judgments that have turned out to be wrong." *Dred Scott*, substantive economic due process, and a crabbed view of the commerce power have been swept away by popular forces that ultimately triumphed in the political process. There is scarcely any comment on the judicial craftsmanship in such opinions. And Chief Justice Marshall's great nationalizing decisions and his opinions establishing judicial power have endured despite criticism of their technical virtuosity.

The results by which Douglas apparently chose to be judged are clear enough. He lent his crucial vote to the expansion of criminal defendants' rights, to the abolition of racial segregation but not to affirmative action, and to the reapportionment of legislative bodies on a strict population basis. In other areas he was an important constitutional innovator. His suggestion that travel was a fundamental constitutional right finally gained acceptance. He established for the Court that the right to vote in state as well as federal elections was also a fundamental right. His vote and his opinions moved steadily toward creating a virtually insurmountable preference for free expression. And the still vibrant and expanding right of privacy, much maligned, took shape largely from his pen.

Four Nixon justices and the majority of legal commentators have disliked his results as well as his reasoning. But his positions have, at most, been only slightly trimmed at the edges. It may well be that looking to the constitutional results, history will vindicate William O. Douglas, who idiosyncratically dignified the Supreme Court by making no pretense to his fellow citizens that it is other than a political branch, by speaking on constitutional issues in an idiom intelligible to the citizenry at large, and by correctly sensing in his opinions the direction of evolving values in the American republic. But if he calculated incorrectly, these two adoring books will save him neither from the adverse judgment of constitutional scholars nor from repudiation of his policies in the political process.

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Reorganizing State Government: The Executive Branch. By James L. Garnett. (Boulder, Colo.: Westview Press, 1980. Pp. xviii + 245. $25.00.)

Reorganization of state government, especially on a cross-state basis, has not been given much attention by political scientists. James Garnett's *Reorganizing State Government* begins to fill this literature gap. As the author himself says, however, much work remains to be done before we have a full understanding of the real situation in this area.

One problem I have with the present volume is trying to determine its audience. In several places Garnett talks about strategies and obstacles for state government reorganizations. Here the question seems to be whether a reorganization effort should be attempted at an early point in a governor's term, or later; and whether such an effort should be comprehensive or piecemeal. The obstacles include such matters as bureaucratic resistance to administrative reforms and resistance of clientele groups affected by reorganization. These are issues that persons involved in reorganization movements must be aware of. Such considerations may lead the reader to see the target audience as government practitioners.

The major part of the book, however, is devoted to an in-depth quantification of reorganization efforts across the country. Table after table, and statistical analysis after statistical analysis, fill the book's pages. This part of the book is obviously aimed at the academic, and is of little value to the practitioner.

Having participated in two reorganizations and having been on the fringes of other reorganization efforts in one particular state, I wonder whether the rather nebulous subject called executive reorganization lends itself to the kind of quantification undertaken by the author. To take one example: is it a simple matter to decide whether a reorganization should be classified as failed or successful? The author seems to feel it is; I do not.

On the positive side, Garnett has undertaken a comprehensive review of all reorganization efforts in all states between 1914 and 1975. This will be an invaluable reference to persons trying to identify efforts in a particular state or in a group of states. Unfortunately, there are omissions, such as the Illinois reorganization study commission report done during the administration of Governor Adlai E. Stevenson, and which received some attention during the governor's 1952 presidential campaign.

This book will be more useful to and more widely accepted by those in academic circles than by those in the real world of state government. Even for the latter group, the survey of reorganizations across the country should be helpful. But, as the author says, this is not the definitive work on the difference that reorganization makes and, more importantly, the policy implications of reorganization. "Without better knowledge than we have now, we are not in the position to determine what difference reorganization makes." It is this question we should be focusing our attention on and it is here that we can be of most help to the practitioner.

SAMUEL K. GOVE

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Limits to Citizen Participation: The Decline of Community Organizations. By Marilyn Gittell with Bruce Hoffacker, Eleanor Rollins, Samuel Foster, and Mark Hoffacker. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 280. $18.00, cloth; $8.95, paper.)

How much influence do lower-income organizations have on school policies in large American cities? Very little, according to this intensive study of 16 local organizations in Atlanta, Boston, and Los Angeles during 1977 and 1978. Having reached a largely undocumented power zenith in the late 1960s, according to Marilyn Gittell and her associates, such groups are now experiencing a distinctive decline.

Several factors contribute to this incipient inefficacy, but class status (measured in terms of economic resources) stands out as a cause. The groups wanted to survive—an understandable desire—and they looked for the means to do so. There waiting with the necessary wherewithal was the federal government, willing to provide funds if the organizations would go into service delivery and get out of policy advocacy. Although middle-to-upper income groups resisted moving from advocacy to service, the poorer organizations accepted the exchange.

In addition to class and its impact on the organizations' redefinition of their own mission, the study identifies other factors—some related, others separate—which limit the groups' influence on both routine educational policies (budget, personnel, curriculum) and episodic ones (e.g., desegregation in all three cities, school safety in Boston, collective bargaining in Los Angeles).

The related factors explaining the decline in the power of community groups involve organizational characteristics: leadership style, membership, and decision-making processes. Service organizations are more likely to be led by paid staff than by rotating elected community residents. This feature inhibits the development of a political force based on numbers, and contributes to collusion between school and neighborhood bureaucracies. In short, as an organization's function moves from advocacy to service, there is less leadership development and more staff cooptation. Moreover, service organizations have less membership mobilization and less internal democracy than do advocacy groups. The organization comes to view residents more as clients needing assistance rather than as comrades working collectively.

Service organizations and advocacy groups also have different relationship patterns with other actors in the educational arena. First, they do not have much to do with each other. Service providers speak to service providers, and advocates converse with advocates. Second, although both types of groups have frequent contact with elected and appointed officials, service organizations' funding dependency and not-for-profit tax status inhibit their lobbying ability.

Even if lack of economic resources did not have such a debilitating effect on these organizations' effectiveness, the authors are not very optimistic about such groups' ability to affect educational policy. For that to happen, the school systems must display "bureaucratic openness." But, according to the results of this three-city analysis, even middle-to-upper income organizations have difficulty gaining access to the school system.

Two other findings deserve mention. First, the media seem to be the leaders and the local organizations the followers in setting the school policy agenda. Second, although there are noticeable differences in economic structure and political culture among the three cities, these variations have only a tangential impact on the local groups. The major findings apply equally to Atlanta, Boston, and Los Angeles.

Gittell and her associates are frankly unhappy about these results. They value participation for its own sake, and they find its diminution a cause for great concern. Realizing in part the naivete of their advice, they urge lower-income organizations to advocate more and serve less—but how then would the groups support themselves?—and suggest that the power holders provide the means for lower-income groups to become more independent—but who would want to finance their own overthrow?

E. TERRENCE JONES

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How Democratic Is the Constitution? Edited by Robert A. Goldwin and William A. Schambra. (Washington, D.C.: American Enterprise Institute, 1980. Pp. 150. $5.25, paper.)

This book, the first of a series in AEI's "A Decade of Study of the Constitution," is similar to the Goldwin-edited *How Democratic Is America?* (Rand McNally, 1969), without its somewhat dated focus on the New Left.

Of the seven essays, three are generally neoconservative. Ann Stuart Diamond synthesizes much of the work of her late husband Martin Diamond in her essay, although she goes even further than he in arguing that the Founders intended to create a wholly democratic constitution.

Walter Berns argues that the Constitution is *not* wholly democratic, since representation is "a way of keeping the people out of government" (p. 65). The central fact, however, is not that the Constitution is or is not democratic, but that it secures natural rights by protecting the private realm (contrary to the wishes of those who would destroy the private realm in a search for radical equality).

Joseph Bessette tries to establish a middle position (less democratic than Diamond's, more so than Berns'): the Founders established a "deliberative democracy." Though it can thwart one kind of public voice, representation meets a hypothetical test for democracy if it can be shown that citizens would reach fundamentally similar conclusions on public policy if they had the same knowledge and experience and devoted the same time to reasoning about legislative information and arguments. Bessette argues that American government has been responsive.

One feels that some of the semantic differences among these positions could be avoided simply by recognizing that there are degrees of democracy. The United States is a "balanced republic": neither an unqualified, thoroughgoing democracy, on the one hand, nor a mixed regime (much less an aristocracy or oligarchy) on the other.

Two of the essays are by historians. Gordon Wood argues that the Constitution was possible, despite fear of central power, because of the desire to counter abuses in the states. The Federalist solution was to erect a government dominated by the gentry (rather than localist "ordinary people") but garbed in democratic rhetoric.

Alfred Young describes the Constitution as an "accommodation." The Revolution's radical democratic impulses dismayed conservatives, who turned to an extended republic to limit state abuses, but were constrained to make democratic concessions at the same time. The Constitution was passed over the opposition of antifederalist democrats, but also with the support of moderate democrats like Jefferson and the democratic urban artisans.

Michael Parenti's essay provides the radical democratic perspective. The tenor of his analysis is demonstrated nicely by his comment (pp. 43-44) on Madison's statement that the government must "preserve the spirit and form of popular government" while guarding against faction: "Here Madison touched the heart of the matter: how to keep the *spirit* and *form* of popular government with only a minimum of the *substance*" (!)

Wilson Carey McWilliams writes from the perspective of classical civic democracy, based on a sense of commonality and equal dignity. The founders sought, not republicanism, but liberty, especially through the weakness of individuals. This weakness, as Tocqueville saw, facilitated majority tyranny, but was once offset by less "modern" facets of American life, e.g., religion, local regimes, and associations (including political parties). The decline of these institutions and the expansion of a public life which neither elevates a citizen's spirit nor supports a citizen's dignity undermines democracy.

On the whole, one wishes that the essays *confronted* each other more, rather than "talking past" each other. Apart from a few scattered remarks, the essays do not attend *directly* to the contrary assertions of other essays (noting areas of agreement, specifying central points of contention, and providing defenses and refutations on those points).

Still, the essays are generally good and they focus on *the* central question for America's understanding of its own political life: whether, and more importantly, *in what sense,* America is democratic. The fact that this question is never "resolved" by such discussions is not grounds for boredom or despair—it is merely a sign of its continual fecundity.

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Energy Policy in Perspective: Today's Problems, Yesterday's Solutions. Edited by Craufurd D. Goodwin. (Washington, D.C.: Brookings Institution, 1981. Pp. xx + 728. $29.95, cloth; $14.95, paper.)

Energy as an area of public policy is beginning to interest a number of political scientists on a professional as well as a personal level. They will find *Energy Policy in Perspective* an indispensable summary of the largely abortive efforts to develop national energy policies in the United States since the end of World War II, and a valuable reference work of continuing usefulness. The volume is organized on the basis of chapters concerning each of the presidential administrations from Truman through Carter (up to the end of 1979). With some variations caused by multiple authorship, each chapter sketches the administrative (and occasionally congressional) initiatives regarding fuels and energy; the various reorganizations relating to energy, not only within cabinet departments but also in the Executive Office structure; and the substantive lines of argument—especially as derived from economic analyses—that have marked the shifting sands of energy policy. One can follow a particular source of energy—oil, coal, nuclear power, synthetic fuels—through its permutations over time, chapter by chapter; and most chapters attempt some modest generalizations about policy options and the overall process, with a particular emphasis on generalization in the final two chapters. An appendix provides a very handy quantitative review of statistics concerning energy supply and use for each of the major sources during the past three decades.

Several themes emerge from a review of the fragmented and sporadic efforts at U.S. energy policy development since 1946. One of the more striking is that for much of the time covered, the United States really did not work toward creating a national energy policy, but only toward a series of individual fuel policies—for oil and gas, for coal and nuclear power, primarily, although with some interesting and abortive early attention (1946-1950) to synthetic fuels by the Interior Department's Bureau of Mines. Despite strong efforts (with the coal industry as a strong supporting interest), the synfuels program succumbed to oil industry opposition and to the apparent passing of urgency with the ending of the Korean War. As one of the authors observes:

> During the time of crisis synfuels development would have required a serious commitment of scarce resources for a payoff that was at least five to ten years ahead, by which time the crisis would probably have passed. During the periods of normalcy [sic], on the other hand, the urgency

> was absent for a resource commitment of any kind (p. 166).

Perhaps a major reason for the failure to develop more than sector-specific policies lay in the absence of an appropriate organizational focus. There was no central policy-enforcing unit for energy until the creation of the Federal Energy Office in late 1973 and the inauguration of the Department of Energy in 1977. With no single office charged with achieving consistency in approach and integration of objectives among competing fuels, the usual centrifugal forces of American politics seem to have prevailed.

The second major theme is the continuing dilemma of national security: whether to import fuel (especially oil) so as to save for emergencies the domestic supply of nonrenewable resources, or to encourage maximum domestic exploration and supply so as to minimize dependence on others. Oil-import curbs initiated by Eisenhower moved in one direction; Nixon's Project Independence attempted to reverse the thrust. Interestingly, it was not until the second oil crisis that Carter (and then Reagan) used price decontrol both to encourage domestic drilling and to discourage demand. Until this time, only an occasional voice in the wilderness had suggested that a one-to-one correlation between economic growth and energy use could be broken, and that energy policy might have demand-reducing as well as supply-enhancing components.

The third and perhaps most pervasive theme has been that when it comes to energy, we have badly failed to practice what we preach about free enterprise. In the words of Craufurd Goodwin,

> The story of energy policy in the United States is largely an account of the failure of genuinely free markets to appear and of the unwillingness of most market participants—buyers, sellers, and government—to allow a free market structure to develop. Despite much rhetoric from all parties on behalf of free enterprise, far more effort was devoted to destroying market freedom if it began to appear than to perfecting it (p. 666).

In terms of presidential administrations, what the Goodwin volume shows is that policy approaches varied more with ideologies of government-business relations than with economic or technological analysis. Truman favored planning, and the Paley Commission report of 1952 pointed toward a substantial resources planning role for the government; Ford wanted the price system to take over the determinative role in energy matters, although the 1973-74 embargo caused a scuttling of that effort. Carter tried the hardest to achieve a national energy *plan,* while Reagan moved quickly to end oil price controls, thus ironically implementing more rapidly a Carter initiative.

Despite these differing ideological predispositions among the postwar presidents, the fact is that interest-group pressures, national security concerns, and (most recently) inflation constraints have consistently produced actions contrary to free-market strategies. Energy policy, as a result, probably cannot now be turned over to the price system and the pattern reversed, for industrial, labor and organized consumer groups have too many stakes in existing nonmarket components of what passes for energy policy.

This leads to an anomalous thought regarding the analysis of energy matters from the viewpoint of social science: economists have a near-monopoly on presumed expertise (second only to physical scientists), yet the United States seems strongly disinclined to let economic rationality determine outcomes. At the least, we seem to need an interdisciplinary mix of economics, interest-group and public opinion analysis, organizational innovation, technological assessments, and multifaceted examination of value tradeoffs implicit in various specific proposals. The present volume, edited and written by experienced academic economists, very effectively relates the chronological story of energy policy formation predominantly from the standpoint of the economic propositions and presuppositions of agencies, presidents, secretaries of the Interior Department, commissions and assorted bureaucrats. It does not, however, employ political concepts and variables in more than an offhand way, so there remains, from the viewpoint of political scientists concerned with public policy, much work to be done. Craufurd Goodwin and his colleagues have given us a splendid starting point.

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Memorandum for the President: A Strategic Approach to Domestic Affairs in the 1980s. By Ben W. Heineman, Jr., and Curtis A. Hessler. (New York: Random House, 1980. Pp. xxv + 404. $17.95.)

Having participated in domestic affairs decision making, Ben Heineman and Curtis Hessler, two former members of the Carter administration, attempt to answer the question: "How could the presidency and executive government be handled better?" (p. xxii). Written before the 1980 presidential election, this volume was prepared as a memorandum to whoever would occupy the presidency during the first years of the 1980s. It is a "how-to-govern" in domestic affairs treatise which was sorely absent in the first year of the Carter presidency. Hence, the work is both prescriptive and descriptive.

The thesis of this text is that in the 1980s a presi-president's resources will be limited and the constraints on a president's powers will be great. Therefore, to guard against failure the president must develop and follow a "strategic approach" to the domestic presidency (p. 6). As the authors inform the president, "The nature and degree of presidential power you will be able to exercise turns on your ability to act strategically" (p. 17).

Developing a strategic approach to the domestic presidency "involves joining policy and politics, structure and process in a coherent plan for four or five years" (p. 16). In creating the strategic approach, the authors draw extensively on what they perceived to be the fundamental mistakes of the Carter administration. In this manner Carter emerges as the negative model of the domestic presidency, much as Franklin D. Roosevelt was the positive model for other scholars.

Planning the strategic approach requires considerable knowledge regarding the environmental constraints on the president (e.g., economy and budget, fragmented Congress, strong interest groups, weak parties, an adversarial press and a cynical public). A keen understanding of the constraints is essential for establishing realistic expectations regarding what presidents can accomplish in their short tenure. Understanding constraints becomes a necessary but not sufficient condition for a successful presidency.

At the very heart of the strategic approach is the need to reconcile the constant conflict between policy and politics. "To be 'right,' but impotent, is not a prescription for success in office" (p. xvii). The skills of the policy analyst and political advisor must be brought together to forge a policy which will effectively address the problem and at the same time be politically feasible.

The problems of structure and process for managing the presidency are given the most attention by the authors. The key element here "is a simultaneous process of centralization and decentralization" (p. 10). The major issues confronting the administration should be classified into three groups: presidential, presidential/secretarial, and secretarial. This typology reflects the degree of presidential involvement which should exist on any given issue. Presidential and White House staff involvement should be the greatest on the presidential-level issues since it is on these issues that the administration will be judged. As one descends, more of the decision-making responsibilities should be delegated to the cabinet officers. Hence, the cabinet becomes a significant element in the strategic approach.

With the cabinet delegated authority in making most sub-presidential decisions, the executive office of the president is available to "guide the development and implementation of presidential-level issues" (p. 11). However, Heineman and Hessler argue that the EOP is currently not designed to play this role effectively and must be restructured. The EOP should be consolidated so that a strong chief-of-staff presides over four primary channels of activity: economic policy, domestic affairs, budget and management and political affairs. Organized in this manner, the EOP will be responsible for four major tasks: process management, advice-giving, decision making and strategic thinking. If these tasks are accomplished successfully, "the EOP will provide a brain and nervous system to the body of the executive government" (p. 188). The authors also suggest that the president must be concerned about other factors such as timing (time is the constant enemy of presidents) and guarding his professional reputation (pp. 29-30).

Two case studies are presented in chapter 5 which very nicely illustrate how the strategic approach could have been applied in the Carter administration to circumvent failures in economic and social policy.

Heineman and Hessler are very realistic when they state that "the strategic presidency does not guarantee success" (p. xviii); however, they go on to portend that "absent sheet dumb luck, failure is assured without it" (p. xviii). They have dealt very effectively with the organizational elements of developing a strategic approach. However, if, as they argue, "the ability to act strategically proceeds from a President's 'personal influence on governmental action' " (p. xvii), then they have not sufficiently considered such significant variables as personality, political experience and popular prestige. In the same manner the authors do not consider the growing interrelationships between domestic and foreign policy and their relationship to presidential success.

Heineman and Hessler present an excellent analysis of the problems facing the modern presidency and integrate their solutions into a pragmatic plan to manage one of the most complex political institutions of our time. This volume represents a significant contribution at a time when it is perhaps needed most.

DAVID G. WEGGE

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The Washington Reporters. By Stephen Hess. (Washington, D.C.: Brookings Institution, 1981. Pp. xii + 174. $17.95, cloth; $6.95, paper.)

Portraying the President: The White House and the News Media. By Michael Baruch Grossman and Martha Joynt Kumar. (Baltimore: Johns Hopkins University Press, 1981. Pp. x + 358. $26.50, cloth; $9.95, paper.)

Their titles would lead one to believe there is much commonality and overlap between these two books. In fact there is surprisingly little. They deal with the same political locale, the same period, and many of the same actors. But the research objectives and perspectives are quite different. Complementary, yes; duplicative, no.

Stephen Hess's book is a look at the workways of the Washington press corps. He investigates the attitudes of journalists toward politics, politicians, and coworkers, the nature of their assignments ("beats") in Washington, their aggregate SES characteristics, and the kinds of stories they produce. His data come from face-to-face interviews and/or questionnaire responses from nearly 500 of the 1,250 identifiable professional Washington reporters, and from an analysis of 2,022 stories appearing in 22 newspapers, all three TV network nightly news broadcasts, and one newsweekly magazine in seven days in April 1978.

Mindful of the virtues of replication, Hess designed his research in part for comparison with and for the purpose of updating Leo Rosten's 1937 *The Washington Correspondents* and William Rivers' 1962 *Columbia Journalism Review* article, "The Correspondents After 25 Years." He finds a continuation of the trend that Rivers noted in 1962: Washington journalists now are professionally very autonomous, feeling almost none of the ideological and commercial pressures from *inside* their organizations that Rosten reported in 1937. There are still pressures of tradeoffs and competitive efforts by policy makers, bureaucrats, and other journalists each to maximize ego-rewards or policy influence—in short, the pressures of American adversary politics in American pluralistic settings.

In this short review of a short good book, only two findings, each with the related author's commentary, will be remarked upon. First, a point that is or should be dear to political scientists: Hess observes that Washington journalists think, on the one hand, *that they ought to* do more interpretive, specialized, and "in-depth" reporting. On the other hand, they resist becoming specialists, resist using documents as sources, and dislike beats that require social science learning (even while they recognize the importance of covering agencies like the FED and SEC). The present Washington version of new journalism, Hess says, is "social science journalism," but "the best journalists always have been storytellers, not theoreticians. . . . Out of the frustration of trying to deal with complexity and perhaps out of a feeling that what they do is less worthy, good journalists may become bad social scientists. The market in bad social science is already glutted" (p. 120).

Hess's second finding and commentary have to do with the political biases of the Washington press corps. By self-rating his sample was 42 percent "liberal," 39 percent "middle of the road," and 19 percent "conservative." Few of his respondents had any strong political beliefs, however. "I am astonished at how many reporters have no political views," said one young journalist. "Hardly any Washington reporters say they participate in community activities, and some make a point of saying they do not vote" (p. 89). What is one to make of this? As Hess says, "An apolitical press corps in Washington, if this is the case, looks like a strange finding." He believes that what draws journalists there is not "a deep interest in political ideas," but "the attraction perhaps is to excitement and powerful personalities" (p. 89). He concludes "that Washington reporters are more apolitical than press critics contend. The slant of Washington news is more a product of the angle from which it is observed than from ideology" (p. 115).

Michael Grossman's and Martha Kumar's *Portraying the President* is a useful but badly organized sourcebook of fact, anecdote, quotations, commentary, and analysis on contemporary president-media interactions. Useful, because it is exhaustive, up-to-date, and based on some five years of diligent work by the authors; badly organized, because it has a Topsy-like organic quality, it is repetitious, and it lacks structure and craft (more on this later); sourcebook, because its principal value, I predict, will be references to it and quotations from it made by students of the presidency and the media, and by generalist teachers who retail bits and pieces of it in lectures and readers. Like Key's *Southern Politics,* or Duverger's *Political Parties,* it may become one of the most cited unread books of our profession. That, if it happens, will be fame aplenty, and an important contribution to scholarship and classroom.

Grossman's and Kumar's notes reveal that they must have talked with every important Washington newsperson and presidential staffer of the last 15 years. Besides this prodigious interviewing, they analyzed a sample of all White House stories in the *New York Times* and *Time* magazine from January 1953 to September 1978, and CBS Evening News stories from August 1968 to August

1978. In what is, I think, their best chapter, the report of their content analyses, they find that (1) these prestige news organizations heavily and persistently put out favorable stories about the presidency, (2) there are recurring patterns of coverage from administration to administration, and (3) there are striking similarities in the *kinds* of stories about the presidency. Their data show that over the whole 25 years the *New York Times* and *Time* magazine ran nearly two favorable stories for every unfavorable one about the White House. CBS stories for 1968-1978 ran about one for one, but, as the authors point out, those were particularly quarrelsome years and the two print sources were also less favorable in that decade. "Even more favorable are the pictures that accompany news stories. There were almost no negative pictures in either the *New York Times* or *Time,* and there were only a few negative films on the CBS News" (p. 256).

With regard to patterns of coverage, the authors find the media analogue of the now-familiar long-term downslope of mass opinion on presidential popularity. "The largest number of stories and the largest number of favorable stories appear during the first year. Rarely does the tone rise after that first year" (p. 259). And they report that "the White House story is a homogenized story. The three news sources show strikingly similar patterns in the types of stories they write and the subjects they cover as well as in the overall tone of their coverage" (p. 265).

One attractive feature of the book is the use of illustrative case studies. They vary in heuristic value, but "A Little White House Ceremony" (pp. 124-29) is especially well crafted. It could be a nice class handout to lead off a lecture on the institutionalized presidency.

I found *Portraying the President* on the whole to be a dull book, partly, as indicated above, because of its inchoate organization, partly because it is repetitious, partly because of its pedestrian prose, and partly because there's too much detail. Did we really need to know that Barry Jagoda's "most famous recommendation was to have President Carter deliver a 'fireside chat' dressed in a sweater to symbolize the need to save energy" (p. 111)? Much of the book is a cross between Ogg-and-Ray and Joe McGuiness.

Chapter 6 provides examples of repetition. It is about "sources," which the authors define as providers of unattributable information, both official and unofficial—attributable informants are somehow not "sources" in this definition. They say there are five reasons for the use of "sources," the second of which is "to build support for the administration" and the fourth of which is "to build political capital." Readers who suspect that building support for the administration is the same thing as building political capital find after six pages of text on the first and two and a half pages on the second that they're right. The authors' single point is that recent presidents and their staffs have tried to use the media, overtly and covertly, officially and unofficially, to improve their images and gain support for their actions—a point that is hardly new and needs to be made only once, with maybe two or three examples.

In the chapter, "The White House Bubble Machine," it would have been useful to have some figures—maybe a table of organization—visually showing how the units of the executive office interact in publicity operations. Some of the tediousness of detailed prose descriptions could have been avoided.

A propos of repetitiousness, superfluous examples, etc.: is there a more general point? Are "presidential studies" flawed by a biographical-journalism (even exposé) quality that make them different from most of the rest of American political science? Must presidential studies be a combination of "great man" biographies, tiresome autobiographies, backstairs gossip, and journalism both good and bad? Even the best—e.g., Neustadt's *Presidential Power*—seem to lack the conceptualization, the hypothesizing, the patient collection of data, the application of criteria of admissibility of data, the discovery of pattern or lack thereof, and the explanations with an explicit or implicit framework of causality. Studies of legislatures, political parties, campaigns, community power, voting behavior, attitude formation and change, political socialization, the courts, public policy making—every major subfield in comparative and American politics is now postdescriptive in its research methodology and its level of abstraction. All except presidential studies, where anecdotes of who suggested that Carter wear a sweater on TV still have redeeming scholarly value. I say this not to criticize Grossman and Kumar, but to ask the question of all presidential experts: will you ever be able to get past biography?

BERNARD HENNESSY

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Black Representation and Urban Policy. By Albert K. Karnig and Susan Welch. (Chicago: The University of Chicago Press, 1981. Pp. xiii + 179. $20.00.)

Albert Karnig and Susan Welch have written an important book which will be an essential reference for subsequent work on black politics and urban governance. Their study provides the first comprehensive analysis of changes in black officeholding in American cities between 1970 and 1978. It analyzes aggregate data on the election of blacks to mayoral and city council seats in 264 American cities with populations over 25,000 that are at least 10 percent black. Two central questions are addressed: What conditions are associated with the election of black mayors and city council members? What influence do these black elected officials have on city budgets? Admirably explicit about the limitations of their data, the authors address these questions competently, concisely, and clearly, presenting thoughtful summaries of hypotheses, descriptions of trends, bivariate analyses, and multivariate analyses.

Their findings about the kinds of cities which nominate and elect black mayors and council members are generally strong and convincing. According to the authors, while black representation has become more equitable during the 1970s, blacks are still represented in mayoral and council seats, respectively, at only 40 percent and 71 percent of the equitable rates. The major factor explaining variations in equitability across cities, say Karnig and Welch, is the level of black resources —that is, the size, education, and income of the black population. Blacks are more likely to become candidates and to be elected in cities where there are larger proportions of blacks in the population and where their SES is higher. Neither white population characteristics nor federal antipoverty efforts had as substantial an impact as black resources. Similarly, most of the political and electoral factors examined by the authors did not have as important an impact on black representation as black resources, but district elections were associated with higher rates of black council representation and partisanship was associated with fewer black candidacies but not with fewer black winners. Karnig and Welch conclude that if blacks can get nominated in partisan cities, they benefit from the partisan system. Interestingly, they found that northeastern cities have lower levels of representation than would be expected on the basis of black resources or the other sigjnificant explanatory variables.

Inevitably, questions can be raised about their statistical analyses of the conditions associated with the election of black local officials. For example, in the absence of data on the number of black candidates for any specific council seat, Karnig and Welch rely heavily on a measure of candidate representation which is a ratio of the number of black candidates to the number of council seats in a given election. They assume that "a larger number of black candidates per council seat is a favorable outcome for blacks" (p. 69). Yet in both district and at-large elections, a higher candidate representation rate may indicate increasing competition among blacks and a decreasing chance of winning seats. Questions can undoubtedly also be raised about the validity of their multistage method of dealing with the problem of multicollinearity, and about the atheoretical character of the analysis. Little attempt is made to elaborate on the possible causal relationships between independent variables.

Karnig's and Welch's treatment of the second question about the influence of black elected officials is not as strong or convincing as their analysis of representation. But their findings do provide a basis for future studies. Their bivariate analysis of 139 cities over 50,000 in population (the only cities in their larger sample for which the census reports annual budget information) shows that cities with black mayors and over 20 percent black council representation increased their total expenditures, social welfare expenditures, and intergovernmental revenues per capita more than did other municipalities. However, the multivariate analysis shows that the effect of mayors is "neither large nor completely consistent [and] even that small effect does not seem to hold for black city council members" (p. 126). Thus the presence of black mayors is found to bring about more consistent increases in expenditures than black council representation, but the increases associated with mayors, "though often statistically significant, are not powerful" (p. 153). However, reviewing the numerous obstacles to translating black representation into policy change, the authors quite wisely suggest that the relationships which do exist may be seen as quite remarkable. And they call for further research on distributional effects which their data cannot detect.

Overall, Karnig's and Welch's book will be important to those who choose to test their generalizations in similar large-scale aggregate data analyses and it establishes useful parameters for those who prefer to delve into the complex political dynamics which are lost in such studies.

DALE ROGERS MARSHALL

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The Vietnam Trauma in American Foreign Policy, 1945-75. By Paul M. Kattenburg. (New Brunswick, N.J.: Transaction Books, Inc., 1980. Pp. xiii + 354.)

Paul Kattenburg was one of the authentic— and, unfortunately, one of the few—government heroes of the Vietnam War. Kattenburg was the State Department's Vietnam Desk Officer in

1952-56 and the Director of Vietnam Affairs in 1963-64. From the outset, he was opposed to U.S. involvement in Indochina—for all the right reasons—a fact which naturally did not help advance his career as a foreign service officer. Now teaching political science at the University of South Carolina, Kattenburg has written a major history and analysis of the Vietnam War.

Kattenburg argues that the American intervention was an outcome of four central factors. First, the victorious war against Japan left the United States as the dominant power in Asia after 1945. Secondly, our military power created both the opportunity and the temptation for armed interventions. Third, our ideological anticommunism overrode our anticolonialism, making the Vietminh's struggle against the French into a component of our own containment policy. Finally, our continuing involvement was a function of *machismo* among top policy makers, particularly but not exclusively in Nixon; the fear of being perceived as weak and a predisposition for dashing action rather than reflection resulted in placing an inordinately high value on the use of force. Kattenburg sums up the factors that led to U.S. involvement as follows: "The United States [went into Vietnam] . . . because of containment, its superpower methods, including its anti-Communist component, its appraisal of the danger of national liberation wars, and its firm belief that U.S. commitments everywhere would be jeopardized if it did not show firmness, determination, and persistence in Vietnam" (p. 184).

The U.S. failure in Vietnam, Kattenburg goes on to argue, was a function of a number of misperceptions. First, we failed to accurately perceive that the Vietnam War was "an internal revolutionary or civil war" (p. 241), in which American military power was unequal to the challenge of the resourceful, highly skilled, and popularly supported NLF. Contrary to official U.S. propaganda, there was no major North Vietnamese presence in South Vietnam until 1965, after the American escalation, and even after the North Vietnamese matching escalation, defeat was more a matter of political than military failure: the fact that the South Vietnamese government and even military structure "remained to the end empty institutional shells with no political content" (p. 207), unable to compete effectively with the NLF. Second, the United States underestimated the staying power of North Vietnam, able to match the United States escalation for escalation, and willing to take the damage and the casualties. Third, the United States exaggerated the role of external communist powers: neither China nor the Soviet Union played a major role in the initiation of the struggle in Indochina, the Soviets were prepared to abandon the cause of a unified Indochina under communist rule in the late 1950s, and it was not until the major escalations of the 1960s that the Soviets moved to substantially back North Vietnam. Finally, the government overestimated the willingness of the American public to support a futile war indefinitely; by the end of the 1960s "U.S. opinion inchoately but unmistakably began to sense that if the administration was unwilling to win, the public itself should not continue to support a fight which it might be disagreeable and inconvenient but in no way fatal for the United States to lose" (p. 245).

Summarizing, Kattenburg writes: "In Indochina, the decision-makers piled misperceptions upon misperceptions: they were unable to distinguish Vietnamese communism from other communisms; nationalism from communism; Vietnam and Indochina from Europe and other Southeast or Asian political settings; civil war and revolution from externally directed interstate war" (pp. 318-19). Why were such fatal errors committed? Primarily, Kattenburg argues, because knowledge of the particularities of Indochina was submerged by "broader" considerations: the decision to oppose communist revolutions generally, the felt necessity of upholding U.S. "commitments," the imperative of demonstrating toughness and "credibility," etc. Thus the United States had very little interest in Indochina as such. In this ignorance and hubris are to be found the seeds of the tragic failure of the United States in Indochina.

It is here also that we must look for the central lesson of Vietnam: "*not* that U.S. security is never involved or that U.S. power should never be engaged; but rather than global approaches and global laws do not work, and that each case must be regarded on its merits" (p. 321). Kattenburg has performed a signal and timely service to his country in demonstrating this so persuasively in his study of the Vietnam War.

JEROME SLATER

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The Politics of Presidential Appointments. By G. Calvin MacKenzie. (New York: Free Press, 1981. Pp. xxi + 298. $19.95.)

The political significance of presidential appointments is substantial. The quality of the men and women selected helps to determine the quality of the work that the federal government does; it also suggests the view of the offices at stake on the part of those responsible for the choice and provides an indication of the administration's policy and programmatic agenda. The nature of the appointments process reflects the nature of the presidency, the Senate, and relationships between them.

G. Calvin MacKenzie has written an institutional and normative analysis of the procedures by which recent presidents have selected appointees subject to senatorial approval and the concerns demonstrated by the Senate in rendering its advice and consent. Principal data were derived from interviews with White House officials, Senate staff, and nominees; hearings on most major nominations from 1964-78; and White House and Senate papers concerning appointments.

In four major sections, MacKenzie both reviews and evaluates the process. He describes development of selection procedures in the presidencies from 1945-77; assesses the intent of the Framers of the Constitution and the patterns of Senate concerns and actions regarding nominations; and notes the participation of political parties, departments and agencies, interest groups, and members of Congress in shaping appointment decisions. He concludes that the process is inherently political but subject to marginal improvements in outputs in terms of appointees' substantive qualifications, administrative competence, and diversity of social characteristics as well as performance of their constitutional roles by both the White House and the Senate.

MacKenzie's book has several strengths. As the only recent treatment of both presidential selection and senatorial confirmation, it responds to a scholarly need. As the author notes, the study builds on analyses by Richard Fenno of Cabinet selection and by Dean Mann of the recruitment of political executives. It also uses the work of Laurin Henry and David Stanley on presidential transitions and Hugh Heclo on the interrelationships (and lack of them) among political administrators. Further, in the tradition of Neustadt, MacKenzie provides a history of the White House personnel operation that links the unique requirements of a particular presidency to the selection of appointees in that administration.

Two aspects of MacKenzie's approach seem especially appropriate. He deals with the full and hence normal range of appointments by the president subject to senatorial confirmation, rather than only the atypical cases causing public controversy that have been studied more frequently, with perhaps misleading results. The author also places the appointments process firmly in the context of policy making generally, thus suggesting its broader significance in the political system as well as its narrower implications as a process of personnel management and patronage. Throughout, the author consistently confronts the reader with the complexity of the process, to his own credit and to the benefit of his audience.

MacKenzie's evaluation of the appointments process also is thoughtful and constructive. It provides a useful contribution to a procedural debate that in the years of Bert Lance and others who preceded and followed him has seemed to fluctuate between cynicism and näiveté.

In focusing on process, however, MacKenzie has chosen not to study appointments as aggregatable cases of behavior. In a methodological appendix, he reports that he rejected a quantitative approach to the hearing data as excessively complicated, incapable of objective categorization, and misleading because content analysis would be unable to distinguish between frequency and salience. The difficulties are real; but it might have been possible to make clear the limitations of the content analysis and to supplement the research with interview and other nonquantitative information.

The result of ruling out any quantitative data is imprecise analysis. The study does not present the dynamics of the appointments process in terms of either explicitly stated variables or relationships among them. When MacKenzie generalizes about the frequency of patterns, he must state them using terms like "occasionally" or "often," rather than more exact language. Methodological issues assume particular importance in the findings of the book's section based on confirmation hearings. The three central concerns ascribed to the Senate in reviewing nominations—constituency interests, appointees' qualifications, and public policy objectives—are derived from nonquantitative review of the hearings and interview data. Quantitative study of the hearings alone would not prove or disprove senators' "real concerns"; but it would provide a useful and precise tool that also might serve as a check on the assertions of participants. Also, the analysis would be enhanced by attention to ranges of opinion and behavior among senators and committees and over time—important variables in that personally and situationally oriented institution.

In all, the author offers useful insights into a process of major importance for political decision making and the outcomes of public policy. His emphasis on those relationships is instructive. In particular, his accounts of presidents and their personnel staffs provide a valuable source for those seeking to understand White House politics.

JUDITH H. PARRIS

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The Library of Congress

Plea Bargaining or Trial? The Process of Criminal-Case Disposition. By Lynn M. Mather. (Lexington, Mass.: Lexington Books, 1979. Pp. xi + 171. $14.95.)

This book is a well-written and well-documented study of the case disposition process in the Los Angeles County Superior Court. During her year long research, Lynn Mather devoted five months to almost daily observations in this court. Her time was well spent in that the volume conveys an unusual sense of reality for this type of project and contains numerous insightful, candid remarks by court personnel.

As its title aptly indicates, one of the primary objectives of the book is to determine case and defendant characteristics which are most often associated with a plea-bargained sentence and others which are linked to a judge or jury trial disposition. Trials are most probable, Mather finds, either when a defendant is likely to be convicted of a serious crime and has a significant prior record or when the prosecution can only muster a weak case based on the evidence. In the former circumstance, the accused, the proverbial two-time loser, risks little by a trial; in the latter, chances of acquittal are relatively good. In both instances, informal or formal negotiations among the attorneys (and occasionally including the judge) either do not take place or they break down.

Not surprisingly, the seriousness of the case is a critical factor in the disposition process. Mather uncovered shared perceptions and general consensus among courtroom "regulars" (public defenders, assistant district attorneys, court staff, etc.) on the elements comprising a serious case. A felony offense, a probable state prison sentence, or an offense by a defendant with prior convictions is likely to result in case assignment to a more senior judge and/or a more experienced prosecutor. In serious cases, the prosecution is actively interested in securing both a conviction and a relatively severe sentence; any plea bargaining that takes place includes explicit negotiations over the terms of a sentence. In a case defined by court personnel as less serious, the prosecution primarily wants a conviction and, through an implicit agreement with the defense attorney, is usually willing to accept a more lenient sentence.

While findings similar to most of Mather's have been reported in a wide variety of criminal court settings, two practices documented in Los Angeles are noteworthy. Contrary to the traditional pattern of case disposition, many attorneys apparently develop pretrial and trial strategies *after* determining the likelihood of a conviction and probable sentence outcome. The implications of this finding are particularly significant if these calculations only apply to certain types of attorneys, defendants or crimes. Further study is clearly in order. Second, many cases end in a "slow" guilty plea by a defendant after an abbreviated and primarily symbolic trial. Court actors engage in the trial ritual but, in order to speed disposition, act under the assumption that the defendant is guilty of some crime.

By demonstrating the importance and value of an in-depth awareness of court activities, the book is testimony to shortcomings of many large-scale computerized analyses of trial court data. Unfortunately, the strengths of *Plea Bargaining or Trial?* are also the source of its weaknesses. Too often, detailed court observations are not placed in a broader theoretical context as individual case histories follow one upon another. The ideal, of course, is a balance between breadth and depth which few studies have achieved. The fact that Mather has not achieved such balance should not greatly detract from what is otherwise a solid research effort.

THOMAS M. UHLMAN

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Politics and the Oval Office: Toward Presidential Governance. Edited by Arnold J. Meltsner. (San Francisco: Institute for Contemporary Studies, 1981. Pp. xi + 332. $18.75, cloth; $7.95, paper.)

This collection of 13 original essays has been touted as "a guide for the new Reagan presidency" (*Los Angeles Times*). It focuses on challenges to presidential governance and offers prescriptions for meeting them. Although the general assessment of the condition of the American polity is rather grim, the authors reject the conclusion that the nation is ungovernable or the presidency unmanageable and argue that vigorous, realistic and informed presidential leadership can restore national political health.

The editor opens the book with a memorandum to President Reagan which summarizes the analyses and suggestions of the contributors, who are a mixture of seven political scientists, two economists, a lawyer, a journalist and a management analyst. The contributions fall into three parts. The first examines the political environment in which the president must operate through analyses of public opinion and popular mood, changing electoral behavior, declining political parties, the "imperial media," presidential-congressional relations, the president's problems with the bureaucracy, and the presidency and the federal courts. The second part deals with the presidential office, the obstacles it presents to leadership and how these might be overcome. The final part explores the current policy agenda regarding the economy, energy and national security, and evaluates alternatives in the context of constraints imposed by the domestic political environment and the presidential office. Meltsner concludes the book with a thoughtful discussion of the relationship between governance and politics. He suggests that the president have "a short list of policy objectives and the will to accomplish them" and urges upon him "a modicum of caution and reflection" (p. 301).

It is not possible in a brief review to do justice to all of the contributors or to offer criticism where it seems appropriate. One essay, however, warrants special attention. Robert M. Entman's "The Imperial Media" attracted widespread reaction in the national press, most of its hostile. Entman suggests that the president and the White House staff keep their distance from the press which he (Entman) regards as more devoted to drama, sensation and trivia than to public enlightenment. Certainly, several recent presidents have been burned by the news media and had execrable press relations. However, the solution is not to tame the media but rather to recognize that the interests of the president, the press and the public are not identical and to proceed to build a more understanding relationship on that basis. Also, had Entman had the example of Ronald Reagan's communicating skills and the Reagan press operation before him when he wrote, perhaps he would not have been quite as alarmed by the impact of the media on the presidency. Entman has, however, provided a tight, provocative commentary on the subject that journalists, politicians and students of politics should not ignore.

Many of the contributors are well known to political science audiences and their essays are primarily restatements of ideas with which their readers are quite familiar. The main themes used to describe contemporary American political life run through several of the essays—an apathetic and distrustful electorate, weak and still declining political parties and a rampant pluralism sparked by single-issue groups. Similarly, the malaise of national governmental institutions—a fragmented Congress, a presidency beset by internal management difficulties, an uncontrollable bureaucracy and an aggressive judiciary—and their inability to collaborate in providing coherence and direction to public policy are concerns that recur throughout the book.

What is perhaps most surprising is the degree of optimism, albeit guarded, that emerges from many of the essays. The message seems to be that while things are difficult, they are not out of control—provided the president is wise, careful, courageous, well informed, politically astute, in rapport with the people, personally likeable, etc. Unfortunately, this is not very likely to happen most of the time. The president is, after all, only human. The major problem with presidential government is that it places too great a dependence on the president. This was quite apparent when, shortly after President Reagan's close brush with death, a Republican member of Congress said to a television news camera that "he was everything. If he had gone it would have blown the whole program." At no point in the book is this basic difficulty dealt with adequately. The perils of personal leadership are real and not likely to be overcome by such devices as cabinet government, super secretaries, enhanced presidential staffing, or additional policy councils. What has yet to be developed is a viable form of collective leadership. Until that happens, we must live with the risks of presidential government. This volume should help us to do so.

NORMAN C. THOMAS

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The Ministry of Culture: Connections Among Art, Money and Politics. By Michael MacDonald Mooney. (New York: Wyndham Books, 1980. Pp. 427. $14.95.)

Public culture in the United States has come under sharp attack by the Reagan administration. While a number of social and educational programs have been high on David Stockman's "hit list," the National Endowments for the Arts and Humanities were near the very top with proposed cuts in appropriations of almost 50 percent. These NEA and NEH budget reductions represent a major policy shift away from government support for the arts. This is somewhat surprising, since these cultural agencies have enjoyed a great deal of public acclaim and have experienced their greatest budgetary growth during the previous Republican administration. What is more surprising is the ready acceptance, if not approval, of these cuts by those whom it would be fair to judge as members of the "cultural constituency."

The writings of Ernest van den Haag and Kingsley Amis in *Policy Review* represent a highly ordered intellectual critique of the assumptions underlying public support for the arts. Michael Mooney's book is quite the other thing. Mooney fancies himself a political muckracker and debunker of bureaucratic pretensions, and his book purports to be an exposé of the NEA, NEH, and public culture in general. Unfortunately, much of the author's valid criticisms of bureaucratic insensitivity and mismanagement are lost in a quagmire of overblown rhetoric and unreasoned generalizations. This includes the repeated use of such undefined terms as "incorporated national culture," the "New Order," "sovietization of literature." Indeed, the very term "ministry of culture" is used continuously but imprecisely throughout the book.

The closest that Mooney comes to specifying this ministry is as follows:

> The arts were declared "the essence of our lives," and therefore were to be regulated by a Ministry of Culture, which included among its powers and patrons NEA and NEH—those guiding lights as Mrs. Mondale had described them—but also the Department of Education, PBS, CPB, and the university of corporation, foundations, private corporations, the agencies of psychological warfare and foreign intelligence, the General Services Administration, the Smithsonian Institution, the National Park Service, the National Science Foundation, the Labor Department, and any other national officer or "expert," as appropriate (p. 145).

That most of the aforementioned are involved in cultural affairs (excepting those unnamed "agencies of psychological warfare and foreign intelligence") is neither a debatable point nor a new insight. At times, Mooney suggests that the Federal Council on the Arts and Humanities is this elusive ministry. But this is without any basis in fact. The Federal Council is a rarely convened advisory body that served as a forum for Joan Mondale's cultural campaigning and for some interagency program reviews; it hardly makes or directs cultural policy. The reality is that there is no ministry of culture. In the United States, public culture, like so many other policy areas, is partial, fragmented, competitive, sometimes contradictory, and anything but the uniform or exclusive preserve of one agency—or even the federal government.

Mooney is not comfortable with the existence of such policy pluralism. He insists that public culture is a governmental conspiracy to use the arts and humanities as a vehicle for thought control. We discover that the basis of this "well-coordinated national culture" is theocratic, not just political. It is a "New Order" that is "opposed to personal indulgences of any kind" as it seeks to impose "the collective altruism of orthodox humanism" (p. 338). On the other hand, no evidence is offered to document how this "humanistic theology" shapes the content of public culture programs.

It is not that Mooney does not have a point when he alerts us to the dangers of a "national culture." What he lacks is a sense of proportion. Much of what public cultural agencies do is liable to criticism, and Mooney is right to point up the incestuousness of the panel system (pp. 319-23), the NEA's secretiveness (pp. 307-09), and the Endowment's poor management practices and arrogance in dealing with the public (pp. 234-45). It seems to shock Mooney grievously (no doubt correctly) that politics intrudes even in so sacrosanct an area as cultural policy, and that cultural bureaucrats can act foolishly, if not downright offensively. But this does not amount to an official conspiracy to impose a cultural tyranny on an unsuspecting public. If Mooney were more familiar with what a totalitarian culture can be like, he would not suggest that "totalitarian" described public culture in the United States.

Anyone who would like to get a good sense of what cultural repression is really like would be advised to read *Testimony: The Memoirs of Dimitri Shostakovitch* (Harper and Row, 1979). Charges of "politicization of the arts" and of bureaucratic mismanagement seem positively insignificant when compared to the Soviet experience where cultural policy is part of the official ideology. The cruel fate of artists in the Soviet Union (and Nazi Germany), who deviated from approved cultural expressions should indicate how trivial are even the worst of the alleged transgressions of public arts agencies in the United States. It is not that the Endowments are without administrative pettiness and programmatic silliness; but the catalogue of sins as recited by Michael Mooney are just that: petty and silly. Critics of public culture could benefit from a comparative perspective that would provide a sense of the dangers of totalitarian culture, but also of how official patronage can encourage culture democracy. Regrettably, *The Ministry of Culture* makes no contribution to such an understanding.

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Naming Names. By Victor S. Navasky. (New York: Viking, 1980. Pp. xxvi + 482. $15.95.)

On February 19, 1981, the *New York Times* reported on the reestablishment of the Senate Internal Security Subcommittee and the possible resurrection of the House of the House Committee on Un-American Activities (HUAC). At a time when American foreign policy is steeped in anti-Communism and the Radical Right attempts to discredit those who reject its ideologies, Victor Navasky reminds us of another time, when HUAC sought to stifle political dialogue, and of the dangers of ideological fanaticism.

Ostensibly Congress can investigate only to gather information in order to legislate or for purposes of oversight, but HUAC, in the 1950s, usually had the information it wanted witnesses to reveal. And while constitutionally Congress itself cannot punish for crimes, HUAC nonetheless accused those who invoked the Bill of Rights—either the freedom of speech clause or the privilege against self-incrimination—of being Communists or Communist sympathizers, charges which found their way to the front pages of the nation's newspapers, leaving in their wake shattered careers, reputations, and scars that have lasted to this day. To be called before HUAC frequently meant the end of one's career unless the witness purged himself by "naming names." The committee, according to Navasky, became a "kind of national parole board" (p. ix) to determine if the witness had truly repented; naming names and providing details of pro-Communist activity became the test of one's patriotism.

Why some people risked ruin and others talked has long been a subject of contention. Most books that cover the McCarthy era do so from the perspective of those who did not talk and condemn those who did, but the situation was far more complicated. Although Navasky's sympathies are clearly with those who chose silence, he is the first author to have interviewed not only those who refused to cooperate but also the informers, and to have explored the reasons the latter named names. He spent seven years researching *Naming Names,* drawing on testimony from the hearings, letters, oral histories, documents, diaries, legal briefs, press clippings, and gossip columns as well as interviewing informers and their children, lawyers, therapists, agents, studio heads and heads of liberal organizations. Repeatedly the acrimony surfaced. Some people refused to talk, or discouraged others from talking about them, taking, in a sense, a "retrospective Fifth." One denounced him as "ghoulish," but most people were willing to talk. Of the latter group, many still justified their behavior, though not always sure they had done the right thing.

Throughout much of history, the informer was seen as an odious creature. American films, until the 1950s, reflected this tradition, portraying the informer, the stoolie, as an outcast. Then, in the 1950s, with the country in the midst of the cold war, the informer became a hero, honored by legislatures and glorified by the media. Navasky carefully explores how this change occurred. He is not concerned with the espionage exposers, such as Whittaker Chambers, who if they were telling the truth deserved to be applauded, or the professional witnesses, recruited by the FBI to join the Communist party and who later moved from hearing to hearing exposing Communists for a fee, often tailoring the facts to fit the scenario. Instead he concentrates on private citizens, people who may have left the party years earlier and who often appeared before the committee because of the threat of blackmail. Navasky focuses on Hollywood because, he contends, Hollywood symbolized the American dream and HUAC, hungering after publicity, wrapped its tentacles around Hollywood.

Not all who cooperated with HUAC did so for the same reason. For those who cooperated willingly, the issue was exposing the evil of the Communist menace. But for others, those who cooperated under duress—people who refused to talk for years, then broke, friendless and often ill, would return to talk in order to work again; the homosexuals who feared exposure; the Jews who were told a Jew who named names would prevent anti-Semitism—and the unfriendly witnesses, the reasons were far more complex. Some in this latter group justified their cooperation by only talking about themselves while others claimed that they did not hurt anyone since they only named names the committee already had. Still others denounced the committee and then turned over names.

Where was the industry—the studios, the agents, the guilds—the support network that might have put HUAC out of business in Hollywood? In supporting the blacklist (and the even more insidious graylist) while claiming no such list existed, studios and guilds alike gave HUAC legitimacy and shaped the way people behaved. In encouraging people to name names or write contrite letters to clear their reputations, the industry affirmed HUAC's values, playing its game and sharing its guilt.

And where was the liberal community? Navasky indicts such pillars of the liberal community as the American Civil Liberties Union and the Americans for Democratic Action for collaborating with HUAC and giving it legitimacy. Paralyzed with fear of being called Communist fronts, these organizations, and others like them, purged their ranks of anti-anti-Communists and proclaimed their anti-Communism from the rooftops. The ACLU, for example, affirmed its anti-Communism by reversing a long-held position that Communists could hold union office, by avoiding the defense of suspected Communists, even by refusing to defend one of its own board members who invoked the Fifth Amendment, and by exchanging information with the FBI. Other liberals, however, refused to play the game; Thomas Emerson, Carey McWilliams and Stringfellow Barr cooperated to form the Emergency Civil Liberties Union to aid those accused of being Communists.

Naming Names shows how easy it was for some of the best and the brightest people in this country to fall into the trap of doing HUAC's work for it.

It should spur our generation to become ever more vigilant in protecting its civil liberties.

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The Mass Media Election: How Americans Choose Their President. By Thomas E. Patterson. (New York: Praeger Special Studies, 1980. Pp. xvi + 203. $9.95, cloth; $8.95, paper.)

"Today's presidential campaign is essentially a mass media campaign." The presidential candidates "direct their campaign activities toward getting their messages through the media as often as possible." For the voters lacking "the benefit of direct campaign contact, . . . the campaign has little reality apart from its media version." With these hypotheses, embedded in his first paragraph, Thomas Patterson reports one of our most complete studies of the role of mass media in an election: it studies the ways in which the voters respond to the media's presentation of the campaign, how much attention voters give it, what they learn from it, and how they are influenced by it. It also examines the election coverage of the press: how it reports the campaign, the candidates, and the issues.

This study is overdue. Campaign managers and candidates are convinced of the influence of the media on voters; the denizens of communication departments and newsrooms assume it; the illuminati frequently chart its influence on the ability of the society to govern itself. Political scientists, on the other hand, have been reluctant to credit the mass media with much influence at all. Among a selected group of colleagues, the public opinion notes of one say very little about the media, those of another (only half-facetiously) indicate that the media have no influence, and a third summarizes several "media studies" searching for even a modest coincidence of conclusions about the impact of the mass media on voters. Patterson's book, in its own right and by its likely influence on future research, will cause a convergence in the discipline's collective understanding of the place of the media in election politics and voting.

At least two features of this research are especially noteworthy. The most striking is the richness of the data. Beginning in January 1976, Patterson repeatedly interviewed a sample of voters in Erie, Pennsylvania, and a second sample in Los Angeles. Some of his respondents were interviewed as many as seven times, some were subjected to as many as five personal interviews up to one hour in length. Complementing these data is an elaborate content analysis of the two major newspapers in each city, *Time* and *Newsweek* magazines, and the evening news programs of the three major networks (each of which has outlets reaching Erie and Los Angeles). If one sets aside the problem of sensitized respondents (which might occur given the frequency of the interviews), the data gathered by Patterson seem to be exactly what one would want in order to study the influence of the mass media on voter decision making. The panel data allow a detailed examination of the responses of voters to the cycle of campaign reporting in the mass media. Coincidentally, the content analysis allows a parallel analysis of how the campaign is treated in the media.

The second feature of this book is that it is not a study of the relationship between the vote and the content of the news, as the respondent recalls it. That is, this is not another (probably doomed) attempt to assess the ability of the media (meaning, presumably, the content of news stories) to persuade voters to select one candidate over another. There is, in fact, not even a chapter on the vote; no attempt is made to model the vote decision as a function of media exposure. *The Mass Media Election* is not narrowly focused on the vote choice. It ranges over several topics; it describes what is covered in a campaign by the media; it documents differences in campaign coverage among newspapers, television, and news magazines; it illustrates the role of the media in creating interest in the election; and it sketches some connections between what the media reports and what the public sees in the campaign. Some of the findings are unexceptional. In this category are such items as the dependence of the level of a candidate's coverage on how well the candidate has done in recent primaries and the dependence of voter information on media content. Others are quite surprising. One example is Patterson's finding that the audience for national political news is increased rather modestly by television. Two-thirds of those who use television as their principal source of news watch the news only irregularly, and as a result television news increases the size of the regular news audience by barely a quarter. Other findings are puzzling, or at least troublesome for political scientists' more simple notions of how voters differ. In this category are data which indicate that highly interested and committed voters are not more likely to recall the substance of the campaign than are low-interest and uncommitted voters.

However, while the book is interesting, it is also incomplete at several points. One of the things it does not do is tie these data to the vote. While Patterson is to be commended for not writing another dreary and frustrated treatise on the influence of the media and the vote choice, no student of elections is comfortable with a book about elections (even if it concerns the media in the election) that doesn't also deal with the vote. That Patterson does not offer such an analysis might leave some readers feeling that the analysis is unfinished.

Another peculiarity of the book is that it is limited to what might be described as "marginal comparisons." Patterson spends considerable time on what the media reports and what the electorate observes, but he spends little effort analyzing the relationship between the two. With all of the data he must have collected on the media habits of his respondents one might have expected a more concerted effort to document the influence of the media on individual voters. However, there is no analysis of attitudes, perceptions, and information differences in terms of the television news, newspapers, or news magazines that his respondents use to follow the election.

Lastly, more than a few readers might wonder if Patterson hasn't begged the question. Nowhere in the book is there an analysis which contrasts media influences with direct mail campaign techniques, personal contacting, and such. On the few occasions when non-media factors are examined, e.g., party identification, they seem to influence the behavior and attitudes of his respondents more heavily than the media variables against which they are pitted. It is not unreasonable to suspect that the influence of the mass media on voters might appear quite modest if it were compared with standing predispositions such as party identification, or interpersonal influences that are coincidental with social networks which lack a manifestly political tone.

Of course, there are always other ways an analysis might proceed. The contribution of Patterson's work is that it, unlike so many studies of the media in elections, is not single-mindedly trying to model the vote with media variables. Patterson sees the mass media as the predominant method of carrying on a modern political campaign, especially at the presidential level. His assertion, paraphrased at the start, that "the campaign has little reality apart from its media version" for both the voters and the candidates, and the analysis he has constructed around this assertion represents a novel research agenda. If voters have become as susceptible to short-term forces as much recent research suggests, a better understanding of the institutions which structure these short-term forces is necessary. Among them are surely campaigns which play to the mass media. Patterson's book deserves a careful reading.

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American Trial Judges: Their Work Styles and Performance. By John Paul Ryan, Allan Ashman, Bruce D. Sales, and Sandra Shane-DuBow. (New York: Free Press, 1980. Pp. xvi + 295. $19.95.)

American Trial Judges is the most comprehensive account of the work environments of lower court judges ever produced. A veritable encyclopedia, the book is based upon the survey responses of 3,032 state trial judges and lengthy visits to three courts. The authors construct a most comprehensive portrait of working judges—their habits, perceptions, activities, and evaluations—and thereby makes a very important contribution to the description of the activity of this important segment of the judiciary.

The research focuses on the *tasks* in which trial judges engage. Rightly criticizing the tendency to examine only a single slice of judicial work, the authors consider six different areas: (1) adjudication; (2) administration; (3) community relations; (4) legal research; (5) plea or settlement negotiations; and (6) affiliation-related tasks (e.g., solidary tasks; group maintenance). The amount of work, its variety, and the nature of judicial tasks are empirically and rigorously detailed. Not surprisingly, the single largest chunk of judicial time is allocated to trial work. Routine activities—administrative work, keeping up with the law, etc.—are common, but do not consume significant portions of judges' time. Occasional duties, such as community or bar relations, add some variety to judicial workdays.

Of course work patterns have implications for other variables, including judges' morale and job satisfaction. Surprisingly, trial judges are far from bored with their work, and criminal court judges are no less satisfied with their work than civil court judges. Generally, however, evaluations of judging are high: judges feel their skills are well used, are highly satisfied with the competence of courtroom personnel, tend to feel in control of their time, and find the demands on their time at least acceptable.

The research considers two major dependent variables—the allocation of time to tasks (work patterns) and job satisfaction. Major independent variables include (1) court structure, especially the size of the court; (2) attorneys' attributes—adversariness, work skills, level of familiarity (as perceived by the judges); (3) the availability and efficiency of support personnel (e.g., court reporters/bailiffs, law clerks); (4) region/political culture; and (5) judicial recruitment and socialization (age and tenure). The authors consider these variables to represent either "organizational" or "individual" influences. The most useful organizational predictor of work patterns is court size, a variable which has both direct and indirect consequences. For instance, in larger courts judges are much more likely to report spending a substantial amount of time on hearing jury trials and on negotiation sessions. This partly reflects the heavy caseloads of urban courts, but also the tendency of urban attorneys to adopt more adversarial postures. Generally, "the work of trial judges appears more intense, time-consuming, and repetitive in larger courts" (p. 229).

The characteristics of attorneys—especially the stability of the workgroups on the criminal side—also have some implications for judicial work patterns. For instance, where workgroups are stable, criminal court judges participate more actively in plea negotiations and spend more time on nonjury than on jury work. While the "workgroup" concept may not be entirely appropriate to civil courts, little impact of attorney familiarity was observed. Other organizational variables seem to have little consequence for the activity of criminal or civil judges.

"Individual-level" influences are investigated through an analysis of the social backgrounds of judges, their socialization and recruitment experiences, their (self-reported) legal skills, and their job satisfaction. It is claimed that the impact of these variables is not great, although some intriguing findings are uncovered (e.g., women and black judges allocate more time to community relations and bar association activity than their male and white counterparts). Generally, most of the empirical findings confirm our hunches and our experience-based speculations (and, of course, confirmation of "common sense" is one of the most useful aspects of scientific inquiry).

Unfortunately, however, it is possible to quibble with the analysis which generates many of the interesting empirical findings. The authors frequently consider extremely small differences in the means of their variables to be substantively important (e.g., pp. 88-89, 130). Very many of the findings are easily attributable to sampling or measurement error (and it is bothersome that standard deviations are so rarely reported). Multivariate analysis is infrequent and, when it is reported, I draw very different conclusions from the tables than are drawn by the authors (e.g., pp. 190-91). In a similar vein, I do not consider a low correlation between length of time on the bench and work patterns to be sufficient to repudiate theories of judicial socialization. And of course the use of self-reports of attributes and behaviors produces obvious measurement problems. Further, one wonders whether it is appropriate to indicate system commitment to plea bargaining by judges' self-reports of the amount of their time spent in plea negotiation. Indeed, many subtle methodological issues are generated by this analysis and readers should be cautioned to consider carefully just what is being measured and how it is being analyzed.

Although this book seems to tell us everything we might ever think to wonder about judging, the substantive importance of the key variables is not obvious. No model is presented of the process through which judicial work styles affect the authoritative allocation of values by courts, which is, after all, the ultimate endogenous variable for those in our trade. It is not apparent how the public policy made by trial courts is determined or constrained by the sort of variables analyzed here. Are there implications, for instance, for substantive policy which flow from a relative emphasis or deemphasis on the six different task areas? It is perhaps unfair to charge that many of the authors' variables are "unimportant," but one is consistently struck in reading the book with the fundamental question: "What difference does it make?"

Perhaps my difficulty in discerning the importance of the authors' variables stems from the absence of a coherent theory to interpret and identify the significance of the empirical findings. To be fair, I'll admit that the authors did not set out to test theory; instead, their work is shaped by an inductive strategy. Epistemologically, the authors are phenomenologists (as clearly explained in an appendix), and they would surely be comfortable with a characterization of their research as "exploratory," their findings as "heuristic." Given the dearth of rigorous, comparative research on trial courts, work of this sort makes no minor contribution. However, the crisis of research on trial courts today stems from lack of theory, not from lack of data. Exploratory research, it must be remembered, is not an "end"; rather, confirmatory research is. Induction must at some point give way to deduction. Yet the whole book reflects a commitment to induction. The book is organized around what judges actually do, rather than around competing theoretical frameworks. When particular data have theoretical relevance, the authors usually call this to our attention, but theoretical conclusions are rare (a rather lame attempt is made in the conclusions to assess whether the findings better support organizational-level or individual-level frameworks but the conclusions stray so far from the data that they must largely be ignored). One can only hope that the time will come when the "insights" of this research are used to make a theoretical contribution to the study of lower courts.

This research should not be taken lightly, as it contains a wealth of data which may be useful for the development of theory. If studied carefully, the book will make additional "exploratory" research on the subject unnecessary.

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Congress and Money: Budgeting, Spending and Taxing. By Allen Schick. (Washington, D.C.: Urban Institute, 1980. Pp. xiii + 604. $27.50.)

Allen Schick is one of the most knowledgeable observers of the congressional budget process currently writing on the subject. Students of the 1974 Budget and Impoundment Control Act will discover a goldmine of information and insight in *Congress and Money.* A participant in budget reform in Congress from the formulation stage in the early 1970s, Schick provides a detailed examination of taxing, spending, and the budget process. This is a book that demonstrates great knowledge and effort. Yet despite the obvious merit of the study, readers may find it disappointing. In the wealth of detail, one finds a poverty of theory. The insider's perspective that provides fascinating examples may have hampered the development of a broader analytical focus that could have made this work a classic.

"The primary focus," the author tells us, "is on how Congress makes money decisions" (p. 10). The centers of attention are the legislative participants—the budget, appropriations, and tax-writing committees, the Congressional Budget Office (CBO) and committee staffs, and the legislators themselves. The objective is to compare current behavior to pre-Budget Act practices, and to examine the patterns of conflict and cooperation that have emerged. Schick's methodology was a scrupulous review of the written record supplemented with interviews of some 90 participants.

Part 1 reviews the "seven year budget war" of the late 1960s and early 1970s that convinced Congress that budget reform was necessary. Schick carefully records the legislative history of the Budget Act and the bargaining and balancing of interests that took place in 1973 and 1974. The Budget Committees themselves are the subject of the fourth chapter which paints the political landscape of the committees, considers the members and leaders, and assesses the rather ambivalent power of the committees. This part concludes with an examination of the CBO and Budget Committee staffs which have greatly expanded the analytic capability and independence of Congress. The treatment of the strengths and weaknesses of CBO is particularly excellent.

The making and enforcing of budget resolutions is the subject of part 2. The four chapters that constitute this section focus on the claimants for resources within the Congress, and the general trends of accommodation found in the actions of the Budget Committees. Chapter 7 tells the story of the politics of resolution-writing in the House and Senate. In the tumultuous House, the Republicans almost unanimously say "no," every vote tests the coalition-building skills of the leadership, and several resolutions failed to pass. In contrast, the process Schick details for the Senate is one of cooperation, bipartisanship, "going along." Resolution-writing is followed by the problem of enforcement—holding the line on additional claims.

Part 3 considers the impact of the Budget Act on the other taxing and spending committees and Congress. Schick has reserved some of his best analysis for the once-mighty Appropriations committees and the Ways and Means and Finance committees. The transformation of the Appropriations committees from guardians into claimants is one of the most significant trends that he identifies. Schick concludes that the Budget and Appropriations committees have been able to work with each other, primarily through mutual accommodation. Budget reform has had less impact on the revenue side of the budget, but he concludes the process has impinged on the tax-writing committees. In chapter 13, "Tax Turbulence in Congress," Schick's review of revenue legislation after 1974 is tremendously valuable. His division of tax policies into three categories provides a useful framework for analysis.

The final chapter is an assessment of the congressional budget process, where Schick stresses the importance of realistic expectations in evaluating impacts. Holding the budget process to a very modest standard of performance, he argues that the most valid criteria for assessment are procedural, not substantive. The process, he concludes, has been accepted, institutionalized, and provides a useful mechanism for the management of budgetary conflict.

The book is logically organized, well written, overflowing with anecdotes and quotes. Schick is a masterful wordsmith with the ability to spark life into material that could easily become tedious. But even Schick's ready wit cannot prevent some portions of the study from becoming bogged down in technical detail. "Everything you wanted to know about congressional budgeting" may be simply too much for some readers. Fewer trees and a better look at the forest would have helped.

While the scrutiny of the public record is incredibly thorough, Schick is not overly fond of citing the existing scholarly literature on Congress and budgeting. Of the nearly 700 footnotes in the book, only a few dozen refer to scholarly books and articles. So what? Schick has cast his work adrift from many important theoretical questions concerning budgeting and Congress as an institution. He fails to follow up potentially exciting theoretical issues. Schick tantalizes us with suggestive ideas such as the notion of the "accommodating budget," but stops short of integrating the parts into a conceptual whole. Why is there a glossary of terms but no bibliography? A work of this nature will be of greatest interest to scholars and seasoned practitioners, not to the neophyte who does not speak the language.

Congress and Money is still an impressive effort. Even without more explicit consideration of executive-legislative power, broader changes in Congress, or impacts on the national policy process, the book will be "must" reading for the serious student of congressional budgeting. That in my opinion he falls short of the standard set by Richard Fenno's classic, *The Power of the Purse,* should not diminish the significance of his contribution. Rather, it reflects the high standards to which a scholar of Allen Schick's caliber is held, and the continuing critical need for theory development in political science.

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Economic Liberties and the Constitution. By Bernard H. Siegan. (Chicago: University of Chicago Press, 1980. Pp. 383. $19.50.)

Should the Reagan administration have its way regarding regulatory and social welfare policies, the charge which Bernard Siegan lays on the federal judiciary will be, from his perspective, less pressing—at least for the time being. Be that as it may, Siegan contends that economic liberties, like political freedoms, are in peril if left to the "seriously flawed" and "infirm" legislative and administrative processes which inevitably result from voter ignorance, apathy and fickleness, the shortcomings of legislators, the lack of accountability of powerful bureaucrats to the electorate, interest-group domination, the need for compromise and expediency (pp. 266, 282). The survival of economic liberties depends on safe delivery into the haven of the Constitution.

This plea for a revival of economic activism deserves consideration. Doubts persist, despite denigration of the Old Court, as to why economic liberties are less worthy of judicial protection than are nonmaterial liberties. In fact, the Warren Court's "new property" rulings (which Siegan regards as edging toward a judicially imposed "guaranteed annual income" and amounting to judicially mandated "income redistribution," pp. 310, 315) signify the close relationship between civil and substantive rights.

Siegan's proposal that the Court regulate the regulators relies heavily on his interpretation of constitutional history and is impelled by his faith in classical liberalism. The Founders linked their preference for "substantial freedom to engage in material pursuits" to the concept of limited government; and expected that the judiciary would, by guarding the one, assure the viability of the other (p. 325). The Court had usefully decided socioeconomic issues for many years; its precipitous withdrawal from the field without benefit of explanation was in violation of its constitutional trust. Special interest lobbyists and reformers, equally insensible of the workings of market mechanisms, have pressed for legislation which has "proved economically harmful" (p. 320). Case studies document the "failure of regulation"; indeed, the "American system is replete with social programs that do not work" (Ch. 12, p. 316). The Court must now assume its rightful role and responsibilities.

Anticipated objections are dismissed by Siegan. Federal relationships will not be altered. What states will lose by the negation of their laws, they will gain by having the Court "limit the federal presence" in their midst (p. 328). Docket burdens will diminish rather than grow. In focusing on the rights of property and on those engaged in commerce, the Court perforce will ignore the "new property" claims of welfare recipients, residents of underfinanced school districts, indigents denied publicly funded abortions—all of whom request that governmental expenditures be redirected or increased in their behalf. "Judges that do not legislate will have more time to devote to their judicial responsibilities" (p. 329). Additional resources required for the "expanding due process coverage . . . might be more than offset by the resultant decrease in the size of government" (p. 328). Whatever harm might result from an "overzealous" Court's disposal of a few necessary laws would be more than compensated for by the reversal of much "adverse" legislation as well as by judicial creation of an atmosphere which would discourage further "shenanigans" (pp. 326, 265).

Arguing that the Old Court's fall from grace was undeserved, Siegan attempts to turn the criticism on its head. Extrapolating from current as well as turn-of-the-century data, he speculates that *Lochner* v. *New York* (1905) must have had the effect of assuring employment opportunities for newly arrived immigrants. He further conjectures that the maximum hours law, if permitted to stand, would have put small establishments out of business, diminished competition and driven up prices—harming the very groups whom regulation is meant to protect. Truths held to be self-evident by the 5-4 *Lochner* majority have been disregarded by those such as Justice Brandeis, whose wrong-headed and overrated *New State Ice Co.* v. *Liebmann* (1932) dissenting opinion did not include Adam Smith among cited authorities. Far from seeking to shelter vested interests, the Old Court attended to loosening economic restraints so that all segments of society might benefit from the free play of market forces.

Siegan suggests that civil liberties jurisprudence provides guidelines for the exercise of the new/old substantive due process. The discrete and insular minorities doctrine is tailor-made for those whom the law, responding to organized pressure groups, bars from market entry. Tests and devices such as clear and present danger, heightened means and ends equal protection scrutiny, less restrictive alternatives and irrefutable presumptions are offered as yardsticks for determining if regulations "benefit society" (p. 322). Government bears the burden of demonstrating a compelling need for laws which restrict economic activity. Parallels between a free press and a free economy are drawn.

Siegan maintains that much regulation is counterproductive, imposing hardships on the poor and powerless. A treatise on how judicial redress might be tendered in such instances is not out of order; and Siegan's first pages hold out such a promise. It is, however, Siegan's grander purpose to supply the Court with the justifications and the jurisprudential principles which will encourage and enable it to reverse the trends of almost half a century. Whatever one's response to Siegan's predilections or argumentation, it is his perception of judicial capacity and function which should give us pause. Even the Old Court had no such illusions or sense of mission.

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A New System for Public Housing: Salvaging a National Resource. By Raymond J. Struyk. (Washington, D.C.: Urban Institute, 1980. Pp. xvi + 263. $20.00.)

Providing suitable housing for low- and moderate-income families is a controversial public policy issue among researchers, journalists, politicians, and academics. While few question the moral value of providing public housing to those truly in need, disagreement concerning the wisdom of various public housing programs is widespread. The debate has for the most part been of two types. Critics focus on mismanagement, the misuse of public funds, and tenant abuses, while claiming that most public housing programs are inefficient and ineffective approaches to the housing problems of the poor. Then there are the strategists, who come forth with new remedies. Both play an important role. The critics fuel the debates, which in turn gives rise to the strategists who have not hesitated to offer their solutions. The work considered here is of the latter type. It draws upon problems associated with projects administered by public housing authorities (PHAs) and offers a new strategy aimed at alleviating these problems.

A New System for Public Housing focuses upon 29 of the nation's 31 largest PHAs, established by the Housing Act of 1937 and administered by the U.S. Department of Housing and Urban Development (HUD). Drawing primarily upon data collected in an earlier study conducted by the Urban Institute, Raymond Struyk uses these 29 large PHAs, rated as "badly distressed" by HUD, to demonstrate two central themes. He argues that "public housing in the largest cities are definitely in trouble" (p. 8) and that what is needed is a comprehensive strategy aimed at alleviating problems and improving PHA management capacity. For Struyk, tampering with one component of the system to the exclusion of others can cause additional problems (p. 211).

Perhaps as outstanding as its comprehensiveness is this study's identification of the many factors/variables that affect the performance of PHAs and to clearly demonstrate their interrelatedness. The problems associated with public housing projects are not treated solely as problems but are presented as factors affecting performance. Struyk makes the simple point that many large public housing projects (not all) have become the slums that they were designed to replace, and that a new system is needed. Factors such as the design of projects, the density of children within a project, the quality of services provided by local governments, the extent of crime in the neighborhoods where projects are located, problem tenants, etc., are examined in terms of their relatedness to PHA performance. Struyk draws interesting conclusions, arguing that while all of the above-mentioned factors can be problematic, and HUD guidelines and regulations notwithstanding, local PHAs have substantial latitude within which to operate but simply have not taken advantage of it (p. 99). In short, problems are not insurmountable and can often be overcome through local-level initiative and good management practices.

Though this study does not blame public housing problems on any one source, HUD emerges as something of a villain. While refraining from direct attack, Struyk examines HUD's funding method for modernizing public housing projects, concluding that it ignores comprehensive planning (p. 97), creates funding uncertainty from year to year (p. 97) and penalizes PHAs that practice good management in modernization (p. 98). Clearly, Struyk's conclusions indicate that HUD policies are somewhat punitive and that in some instances they represent disincentives. He concludes that "HUD initiatives" (programs aimed at improving PHA performance) have been generally ineffective because HUD has chosen to use demonstration projects to test new initiatives, has conducted these projects in isolation (selected PHAs were used), and has failed to combine the best elements of each initiative into a broad program conducive to improved management capability for PHAs in general. To a significant extent, the findings of this report indicate that when HUD follows a selected project approach and dumps massive amounts of aid into selected projects, positive results accrue, but only for the short term. The selected projects will improve, given the massive infusion of funds, but knowledge gained from these initiatives are not applied across the board to improve management in general.

Struyk's suggestions for improved management of PHAs are consistent with his comprehensive approach. Struyk addresses a new strategy by introducing several radical and clearly unacceptable policy alternatives, among them, local government administration of public housing (p. 188) and PHAs as marketplace competitors (p. 189). These suggestions, which are clearly food for thought, are followed by a new system that is indeed worthy of consideration.

The major elements of the "new system" are a reduced role for HUD and a new funding method that would transfer funds to local PHAs on a fair market rent (FMR) basis. FMR is the cost of renting adequate housing in the marketplace. Such an approach would afford PHAs the opportunity to operate in a manner similar to their private sector counterparts. The new system is incentive-based. Savings from improved management of local PHAs would be used for other project-related purposes, usually modernization. Some other elements of the new strategy are as follows: massive increases in funds for severely distressed PHAs, bringing them up to minimum standards; collecting a data base by HUD sufficient to identify problems; streamlining the entire program, removing restrictive regulations and thus allowing for comprehensive planning; and relying less upon demonstration projects by HUD, thus allowing for a greater use of scarce resources.

A New System for Public Housing enables us to examine the problems encountered by PHAs in their attempt to provide low-cost public housing for the nation's low- and moderate-income families. It delves into the problems and difficulties encountered by these authorities and provides us with a very comprehensive analysis of the complexities of such an undertaking. Unlike other public housing studies, it does not write off public housing. It is positive because it examines the system objectively, and says that we can, through good management, salvage this national resource. *A New System for Public Housing* is a first-rate piece of public policy research, containing practical policy alternatives worthy of consideration.

DONALD JULIAN REAVES

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Environmental Politics and the Coal Coalition. By Richard H. K. Vietor. (College Station: Texas A&M University Press, 1980. Pp. xiv + 285. $19.50.)

Abundant but environmentally pernicious, coal has become a controversial component of U.S. energy policy. Over the past two decades, two broad groups have opposed each other: coal interests seeking to expand coal use and environmentalists pressing for controls over such activities as surface mining and coal combustion. Out of this conflict has emerged the framework of environmental policy toward coal represented in such legislation as the Clean Air Act (1970) and its amendments (1977) and the Surface Mining Control and Reclamation Act (1977). In this study, Richard Vietor examines "the evolution of federal environmental policies relevant to coal energy and the political processes that guided that evolution" (p. 4).

Vietor's thesis is that this evolution represents a new "modernized" stage in the relationship between business and government, one which is inimical to democratic decision making. He argues that in response to growing environmental opposition, coal interests—including coal producers, mining equipment manufacturers, electric utilities, railroads, metals producers, commercial banks, and petroleum companies with newly acquired coal holdings—have formed a united coalition which pressures for coal development through lobbying arms, public relatins campaigns, and government-industry "advisory councils." Because of its extensive resources, this "coal coalition" often exercises a near-monopoly over technical information during the formulation and implementation of coal-related legislation. Particularly important in Vietor's scheme is the implementation phase, in which agencies such as the EPA must establish environmental regulations and standards called for in federal legislation. It is here, in informal consultation with federal agencies, that the coal coalition can employ most effectively its technical resources and shape compliance standards. One result is that environmental policy making has become increasingly complex, specialized, and secret, and thus removed from public accountability. The author's conclusion is that "for two decades, the coal coalition has considerably blunted the impact of the environmental movement, and has rendered obsolete some of the rationale for democratic pluralism" (p. 57).

After examining the evolution and structure of the coal coalition, Vietor develops his case through carefully researched studies of surface mining and air pollution policies. Drawing on government sources (legislative hearings, agency files) and coal industry reports, the author fashions a lucid and comprehensive history of these immensely complex policies. Problems arise, however, in reconciling this history with the thesis outlined above. For example, if the coal coalition has become the powerful force Vietor claims, how can one explain the passage, against the fervent opposition of this coalition, of a rigorous surface mining bill in 1977? It is incorrect to assert (p. 124) that the law was not a clearcut victory for the environmental movement; certainly it was. Vietor does concede at least partial victory for environmentalists, and then, surprisingly, explains it by essentially *equating* the two coalitions: "The political activities of both the coal and environmental interests were highly organized, nationally orchestrated, and centrally coordinated" (p. 126). His case history of surface mining legislation supports this statement, but then what of the supposed power of the coal coalition to determine legislative and regulatory outcomes? How does this render obsolete "democratic pluralism" (a term never fully defined)? Finally, if the "environmental coalition" has achieved at least periodic parity with coal interests, why is this coalition given only cursory examination throughout the book?

The case of air pollution policies more clearly supports the author's thesis, as the coal coalition successfully intervened in various administrative settings to weaken environmental standards. Again, however, coal interests were far from invincible; for example, they did not succeed, against a legal challenge by the Sierra Club, in preventing the principle of "no significant deterioration" from being applied to regions with pristine air. And again, what needs to be explained is the relative success of the environmental movement in the 1970s in establishing a set of laws and regulations that provide important protections for the environment.

That Vietor does not fully address this issue and that his explanatory framework—in particular, the confusing uses of the concepts "modernizing process" and "democratic pluralism"—remains incomplete, should not detract from the value of this study. As a meticulous historical record alone, it clarifies many of the recent conflicts over environmental policy related to coal use. As a possible guide to the future, it may prove prophetic as the Reagan administration seeks to "redress the balance" between energy production and environmental protection.

W. RAND SMITH

Lake Forest College

Improving Prosecution?: The Inducement and Implementation of Innovations for Prosecution Management. By David Leo Weimer. (Westport, Conn.: Greenwood Press, 1980. Pp. xiv + 237. $25.00.)

In *Improving Prosecution?* David Weimer examines two of the many management tasks local prosecutors face: organizing and assigning their personnel to handle the caseload, and setting and enforcing office policies. The book seeks three audiences: prosecutors and public officials who fund their offices; scholars who study the criminal justice system; and students of the public policy process, particularly those with an interest in the diffusion of innovations in public agencies.

Two recent innovations in organizational structure and operation receive brief description and analysis in an early chapter (enhanced case screening units and procedures requiring assistants to get prior approval from supervisors before dismissing or plea-bargaining a case). But the major emphasis of Weimer's research deals with the development, diffusion, and implementation of Case Management Information Systems (CMIS) by prosecutors' offices. A CMIS is "a computerized data processing system that collects, on a case-by-case basis, data describing defendants, their cases, and the way their cases have been handled" (p. 46). In theory, a CMIS enhances a prosecutor's management capabilities by allowing him or her to use the data base to detect deviations from stated policy, to identify assistants and branch offices straying from office norms, and to provide feedback to assistants on case outcomes.

Weimer focuses on the CMIS known as PROMIS. Developed initially in the late 1960s, PROMIS underwent several stages of development before LEAA designated it as an "exemplary project" in 1973. At the time Weimer concluded his research in the spring of 1977, LEAA had disbursed $4.7 million to nearly 100 jurisdictions to help finance the implementation of PROMIS. LEAA gave almost $850,000 to a Washington-based, nonprofit research shop (INSLAW) to refine PROMIS and to provide technical assistance to the jurisdictions adopting it. The evaluation of the implementation and performance of PROMIS in the 20 jurisdictions that had operating systems by the end of 1976 constitutes the core of the book.

Had Weimer chosen to report his conclusions of this evaluation in a journal article, he might have succumbed to the temptation to entitle it "PROMIS's Promises." Though his language is gentle, his conclusions are harsh. Three years after LEAA launched the PROMIS transfer project, some of the crucial benefits promised had not been realized in *any* of the 20 jurisdictions. Persistent problems with data quality prevented its use as an effective prosecution management tool. Further, a perverse "catch-22" limited its impact. Only jurisdictions with strong leadership and extra resources to devote to insure accurate data entry realized some of PROMIS's promises. But these same jurisdictions already were the best equipped to solve management problems without the marginal benefits of PROMIS. Finally, Weimer argues persuasively that LEAA prematurely designated PROMIS as an exemplary project, responding to bureaucratic incentives without bothering to conduct an adequate evaluation of the benefits derived from its actual operation.

Weimer's charge that PROMIS's diffusion was premature could be applied to his evaluation of it. His field research ended not long after PROMIS was introduced in many of the 20 jurisdictions, and considerably before the book's publication in 1980. Though the book is well written and the analysis sophisticated and sound, its value to its three intended audiences is limited. By its publication in 1980, many practitioners had already decided to adopt PROMIS. Students of criminal justice will find useful information here, but may wonder whether one or two journal articles might have sufficed. Students of public policy will find a competent description and analysis of a botched effort to induce local governmental agencies to adopt an over-sold innovation, but one that offers few surprises or new theoretical insights.

JAMES EISENSTEIN

Pennsylvania State University

Comparative Politics

The Fifth Republic at Twenty. Edited by William G. Andrews and Stanley Hoffmann. (Albany: State University of New York Press, 1981. Pp. xvii + 521. $34.00.)

The Impact of the Fifth Republic on France. Edited by William G. Andrews and Stanley Hoffmann. (Albany: State University of New York Press, 1981. Pp. xv + 350. $7.95, paper.)

This book, published both in hardback and an abbreviated paper edition, is about power, authority, legitimacy, public morality, the public interest, and republican institutions in times when decision-making structures are under enormous stress because of the revolution in values which Western society has bred and nurtured since World War II. It is about how France, historically one of the key articulators of the major premises of Western society, has undertaken to structure that revolution and handle that stress, particularly within the framework of the Fifth Republic.

It is not surprising that Americans should have taken the initiative in such a study as this. France seems very much to be America's Other, and to serve with us as the primary integrators of the evolution of the modern world. Even though in 1958 much of the American news media cast a tone of suspicion of fascism about General DeGaulle's return to power, the continuing exchange of values and techniques between the two nations, while often explicit, is more fundamentally of that subtlety that insures lasting effect and that confirms the intimacy of the relationship.

What this book conveys before all else is the dynamism that marked the appearance and at leat the early years of the Fifth Republic. In retrospect, that dynamism not only evinced the power of the values that emerged in the French Resistance period, but also anticipated the appearance of a new sense of the person and of society throughout the Western world and the accompanying search for new categories of analysis and organization.

The format of the book in itself practically guarantees success, for the specialized essays are organized between a keynote address and subsequent commentary by one of the principal formulators of the Constitution of 1958, Michel Debré, and a concluding synthesis by Stanley Hoffmann.

Debré's remarks are a restatement and reassessment of the principles of the 1958 constitution understood as the structure of the political mechanism, and considered principally out of a concern for the "vitality of institutions." Debré's perspective is, as might be expected, that of the Resistance leader reflecting on institutions and social spirit in the context of a society conscious that the old values of the nineteenth century and before can no longer serve as points of reference for either social or personal action, and sensing the emergence,under the solvent action of a Cartesian doubt, of new directions, new priorities, new emphases. His contributions are well worth reading for their rendering of French institutions in a historical mode.

But Tocquevillian analyses (and the most satisfying essays of the book have a strong Tocquevillian cast) always bear the dialectical mark of provoking one to analyzing the present moment in the perspective of a future that prolongs the past while creating a moment of surprising frankness that turns its back on the past.

Stanley Hoffmann works within the spirit of Debré's analysis, but projects it forward to estimate the political and social situation emerging in France in the 1970s and 1980s, to evaluate the capacity of existing political and social institutions to redefine themselves while maintaining their nature, and to assess the chances of legitimate institutions emerging to provide the indispensable psychic benchmarks we expect of a Western society. While he describes his effort as simply that of bringing together the threads of the other essays, no one will be surprised to learn that Hoffmann's experience, erudition, and rhetorical skills produce an indispensable synthesis on its own, empirically solidly grounded and conceptually provocative.

Each of the essays, grouped in political, economic, cultural, and foreign and defense policy categories, will inevitably be evaluated within the Debré-Hoffmann framework. Each one is ably done, and several are particularly worthy of note.

The essay by Bela Balassa on the French economy under the Fifth Republic, is already the winner of a Prix Rossi. In addition, Crozier on Gaullism and cultural revolution, Suleiman on administrative reform, Stephen Cohen on the Gaullist economy, and Georges Lavau on the parties of the left seem to me to provide especially stimulating assessments of direction. Of course, the several essays on political parties do not have that ready and steady foundation on which to build that is the given of the analyses of the primary institutions such as the administrative establishment. It is not surprising therefore that they exhibit a greater tentativeness and other marks of the difficulties of conceptualization in this area, as is true of conceptualization about contemporary American, British, and German political movements. They nonetheless deserve close reading.

Certainly all those members of the profession with a particular interest in French politics will want to work through these studies thoroughly. Those who teach French politics will find the whole book or individual essays valuable reading for both undergraduate and graduate students.

The paperback version drops a number of essays contained in the hardbound edition. The section of the original on political party life has been especially affected, and several of the essays on the economy also are missing in the paperback, including Cohen's.

ALBERT A. MAVRINAC

Colby College

Political Anthropology Yearbook I: Ideology and Interest: The Dialectics of Politics. Edited by Myron J. Aronoff. (New Brunswick, N.J.: Transaction Books, 1980. Pp. 217. $29.95.)

As a specialized field of research, political anthropology was securely established with the publication in 1940 of *African Political Systems* by Evans-Pritchard and Fortes. Although the field remains most closely associated with African studies, political anthropology has made significant contributions to area studies throughout the world and to the development of political theory more broadly. Its practitioners continue primarily to be anthropologists who engage in the field research of participant-observation, but few political scientists specializing in the area of the Third World have been unaffected by its perspective.

It has been more than 20 years since Almond, Apter, and Easton brought political anthropology within the broad embrace of comparative political science. Its influence has been both methodological, especially in its structural-functional approach, and substantive, in its concern for the organization and behavior of such basic political forms as tribes, communities, and factions. From its early focus on "primitive government," political anthropology has opened its lens to the processes of political change.

Myron J. Aronoff's *Ideology and Interest: The Dialectics of Politics* is the first volume of the

Political Anthropology Yearbook. The series is designed to provide a forum for an interdisciplinary dialogue. The dust jacket proclaims its intent: "Inviting definitions of 'politics' that are wider than what is conventionally accepted in political science, and encouraging the application of anthropological concepts to phenomena not traditionally studied by anthropologists, the yearbook hopes to foster creative collaboration among the disciplines concerned with understanding political man."

The seven essays in this first volume explore the relationship between culture and politics in the manifestation of ideology and interest. They examine the premise that political behavior is incomprehensible without an understanding of the shared meanings that inform it. Ideology is that definition of reality by which political actors explain and justify their behavior, but there are alternative definitions of reality and there is, inevitably, a gap between the symbolic universe of shared meanings and the social and political reality to which they refer. "The general lack of a perfect 'fit' between ideology and changing socioeconomic and political structures," Aronoff writes, "provides the basis for the dialectical interplay between ideology and interests which is the thematic focus of this volume" (p. 15).

In the great debate as to whether ideology is cause or effect, the contributors bring two basic approaches. Harvey Blustain and Joyce Pettigrew take an essentially materialist approach. Blustain, in his study of caste, ideology, and power in a village in central Nepal, argues that ideologies are to a great extent determined by the context of political and economic relationships. Pettigrew, from the perspective of critical theory, examines the discrepancy between Sikh political ideals and practice. She rejects the Sikh account of their political activities and seeks explanation in the structural position of the Sikhs as a communal minority between Muslim and Hindu majorities.

Myron Aronoff, Peter Bertocci, Marvin Davis, and Frank Manning assume the middle position characterized by the work of Clifford Geertz, among others. It denies a priori primacy to either ideology as a culture system or material power interests and takes the reciprocal or dialectical relationship between ideology and interest as the most appropriate focus for analysis.

Bertocci examines how culture systems interact dynamically with politics in the context of religious corporate groups in Bangladesh. Davis, in his essay on action and meaning in rural West Bengal, calls for increased attention to political culture in its role of directing and justifying action preferences. Manning provides an ethnographic account of revivalist politics in the 1976 Bermuda elections and examines the use of the symbols of revivalist Protestantism by the Progressive Labour party to portray the campaign in religious terms and to foster racial cohesion.

The final essay in the volume, by Martha B. Olcott, is less concerned with the idealist-materialist debate and addresses the transformation of political culture in Kazakhstan. She examines the mixed success of the Soviet Union's attempt to fabricate a Kazakh Soviet culture, nationalist in form and socialist in content.

Altogether this is a fascinating collection of articles, well worth the attention of any political scientist interested in ideology from an anthropological perspective.

ROBERT L. HARDGRAVE, JR.

University of Texas, Austin

Policy and Politics in Britain: The Limits of Consensus. By Douglas E. Ashford. (Philadelphia: Temple University Press, 1981. Pp. xv + 330. $19.95, cloth; $9.95, paper.)

Douglas Ashford focuses on why Great Britain has fallen on relatively hard times, and particularly how the functioning of the British political system has contributed toward that end. To accomplish this purpose, the author combines a series of extensive essays with sets of edited readings that complement the essays. The book begins with a general discussion of the theory or argument to be advanced, and then turns to illustrations of the argument taken from a wide sweep of individual issue areas: administrative reform, economic policy making, industrial relations, local and regional policies, social security, and immigration.

The book's title indicates a main line of Ashford's argument. It is that consensus within the British elite about the inner workings of government and widespread support for British political institutions within the public combine to make the institutions work less effectively than if consensus and support were more moderate. Elite consensus and public support are so great and reinforce each other to such a degree that, in effect, the political institutions are not evaluated by actual performance. As a consequence, the outputs of the political institutions often bear little relation to the inputs directed toward the institutions. This is because the level of elite consensus and public support works to insulate the institutions so well that what is accomplished does not need to bear much relation to the effort that went into it. For example, Ashford argues that an adversariesl style of politics in Britain permeates the realm of parties and voters (that is, the realm of inputs), but has little to do either with how the political institutions decide upon and define the goals of policies or with how the institutions deal with structural problems. It is like schizophrenia: a political system with a split personality in which the one side of the personality has little contact with or effect on the other side. In addition, policy making can be confined to relatively few actors with the result that it escapes the ventilating and sharpening process that would occur were policy making exposed to a greater variety of vantage points.

This is an intriguing line of argument. But there are flaws that make the argument less than fully convincing. One is that the critical variable, "consensus," is not precisely defined as a concept. The author writes of "elite consensus about the inner workings of government" (p. 8). But exactly what set of attitudes or beliefs does consensus about "the inner workings of government" refer to, and to what does it not refer? What are the parameters of the term? Nowhere is this question explicitly addressed. Instead, the central term of the book remains elusive. To complicate matters further, the author appears sometimes to use the term to show opposite effects. For example, when he writes about the economy, he argues that agreement or consensus among the major British parties helps to explain Britain's economic stagnation and misfortune (p. 114). Later, in discussing industrial relations, he claims that it is the absence of an underlying consensus that is the problem; the lack of consensus about the aims of economic policy has undermined progress in industrial relations in Britain, unlike what has happened in Sweden or Germany (p. 156).

The thesis of the book involves an argument worth reading and the series of detailed case studies to be found in the book are interesting. The book raises valuable questions for research to address. But because its central concept remains imprecisely defined, and sometimes may even be at cross-purposes, the book is less conclusive than it might otherwise be.

JOHN E. SCHWARZ

University of Arizona

The Unexpected Rebellion: Ethnic Activism in Contemporary France. By William R. Beer. (New York: New York University Press, 1980. Pp. xxxii + 150. $18.50.)

Those interested either in France or in the revival of ethnic politics throughout the Western world have been waiting for a book with a title like this one for a long time. Although France has not produced minority movements as powerful as those in Great Britain, Canada, or Belgium, there have been rumblings from the peripheries for at least the last 15 years which have gone largely unnoticed by English-language analysts. Unfortunately, if we really want to understand those movements or what they tell us about ethnic politics in general, we'll have to wait some more.

Beer's book is by no means a disaster. His historical treatment of minorities in France is well done. In the first half of the book, he clearly shows that there have "always" been dissident movements in the seven peripheral regions he considers—Brittany, Corsica, the Basque country, Catalonia, Flanders, Alsace, and Occitania. And, for the uninitiated, Beer does a first-rate job of tracing the ebb and flow of such movements over time and across the seven regions.

Furthermore, Beer has *begun* articulating an intriguing argument about what he terms the unexpected rebellion. Basically, he believes that ethnic activism in the modern West stems from a series of factors, including the decline of the nation-state, the rise of supranational institutions, the modernization of the formerly backward areas, and the impact of all of that on young people who seek some form of outlet—political or otherwise—for a rekindled sense of ethnic identity.

I stress the word *begun,* because that's really all Beer has done. The problems with the book, then, revolve around the facts that the theory itself needs more conceptual work and that it is poorly substantiated by the data presented on the contemporary ethnic "scene" in France.

On the theoretical side, I really only have one objection: Beer gives far too much attention to relative deprivation. In recent years, scholars such as Charles Tilly and Theda Skocpol have effectively shown that relative deprivation theories are too apolitical because they do not consider the forces that turn vague feelings of discontent into active protest. In this case, any theory of the ethnic revival must also consider the actions of the state, other protest movements, and ethnic organizers themselves in determining the form, extent, and impact of protest movements.

Empirically, the book is much more severely flawed. In Beer's defense, one must admit that any positivistic social scientist would have trouble with French ethnic politics since the groups are so small and scattered throughout the country. In other words, without a major investment of time and money, it is unlikely that anyone could carry out a satisfactory study of the groups. Nonetheless, there are glaring methodological and substantive problems with Beer's analysis that could have been avoided.

First, Beer does not give many of the critical elements of his own theory adequate attention. To cite but one example, he does not include much on European integration because the book is based on surveys of French ethnic groups and analysis of election returns.

Second, much of the evidence presented often makes little theoretical or methodological sense. While Beer did survey leaders of the major ethnic movements, his questionnaire only covered the individuals' background and current status. Obviously, we would have liked more material on what those people think and do.

Along these same lines, Beer uses the 1974 presidential candidacies of Guy Héraud and Claude Sebag as indicators of support for nationalist movements. Unfortunately, neither candidate was really a nationalist appealing to the disenchanted members of the minority groups. More importantly, the two candidates won less than 50,000 votes between them, hardly representing the true strength of peripheral nationalism in France. In that sense, support for the two candidates can only be seen as too crude an indicator of support for ethnic politics.

Third, Beer just does not ask the proper question. To be sure, ethnic protest in France does exit and comes as a surprise to many observers who once saw France as one of the most integrated Western countries. But, Beer overstates the importance of that rebellion. Once one glances in almost any direction from the hexagon, one is struck by the weakness of the French movements, especially outside of Corsica and Brittany. If that is the case, then it is important to ask why the French have not experienced the rise of new political parties, let alone the turmoil, such as have developed in the peripheries of Great Britain, Canada, or Belgium. In short, the question is not why the unexpected rebellion has occurred, but why it has been so limited.

All in all, then, this is a disappointing volume. For those of us with little background in French ethnic politics, the first half of the book is most informative. But, for those of us looking for a sophisticated analysis of contemporary minority politics in France—especially from a comparative perspective—Beer's book just doesn't make it.

CHARLES HAUSS

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The Domestic Context of Soviet Foreign Policy. Edited by Seweryn Bialer. (Boulder, Colo.: Westview Press, 1981. Pp. xviii + 441. $35.00, cloth; $15.00, paper.)

This volume grew out of a research undertaken by the Research Institute on International Change at Columbia University for the U.S. Department of State. Like many such works, it emerges as a comprehensive survey of the multiple domestic influences on foreign policy formation and offers the most sophisticated commentary available in the contemporary literature.

The individual essays are organized under the subheadings of "Culture and Ideology" (Adam Ulam on Russian nationalism, Franklyn Griffiths on ideology, and Hannes Adomeit on the impact of consensus and conflict), "Politics and Society" (Grey Hodnett on leadership politics, Thomas J. Colton on the military, Jeremy Azrael on the nationality problem, and Walter D. Connor on mass expectations and regime performance), "Economics" (Herbert S. Levine on economic development and technology transfer, Warren W. Eason on demographic trends and labor supply, Morris Bornstein on economic growth, and Arcadius Kahan on agriculture), "Eastern Europe" (Paul Marer on the region's economy and Andrzej Korbonski on political issues), and two final overview chapters, the first and most ambitious by Alexander Dallin and a less helpful commentary by Bialer.

The individual substantive chapters are thorough examinations of their respective topics, although for the most part, they reiterate conventional wisdom rather than break new ground. Most helpful, in terms of sketching clear patterns of policy making and their immediate institutional inputs, are the chapters by Hodnett and Colton, while Connor's effort is the most useful in suggesting the growing importance of the regime-society relationship. One must wonder, however, at the wisdom of including two chapters on Eastern Europe and the absence of a more detailed examination of other issues such as Sino-Soviet or American-Soviet ties as inputs having high salience.

Despite the diversity of the essays, several recurring themes appear. Virtually all commentators on the USSR conclude that the linkages between domestic and foreign policy issues have intensified in the post-Stalin era, some attributing this intensification to the natural evolution of any mature industrial society, whille others explain it through reference to the increasing clarity of the economic and political tradeoffs necessitated by interlocked domestic-foreign policy issues or through the cautious consensus-oriented decision-making style of the Brezhnev regime. Yet another common conclusion is the increasing institutional and informational complexity of the foreign policy-making environment itself; whether a product of normal evolution or a conscious strategy on the part of Soviet leaders, these developments have both increased the number of viable bureaucratic and other institutional actors and vastly improved the amount and quality of policy-relevant information available to them. Third, in differing ways, many of the authors note the continued importance of Russian nationalism, whether conceived as the now pragmatic operational code that conditions Soviet behavior or as coherent "imperial nationalism" that informs Moscow's actions.

This work's most severe limitation lies in its inability to advance the discussion of the theoretical issues raised in any examination of domestic and foreign policy linkages. Admittedly, its purposes were essentially nontheoretical: to provide, for State Department and eventually area specialist consumption, a wide-ranging overview of the many contributions to Soviet foreign policy formation. Yet, with the exception of Dallin's essay, no attempt has been made to advance theory beyond the mere listing of input categories. In its own way, Dallin's piece well illustrates both the opportunities and difficulties in such theorizing. While a real contribution is made in discussing the institutional "quasi-accountability" of the Soviet foreign policy-making process and in pointing out the usefulness of the issue clusters approach ("right" and "left" emerge as meaningful categories, linked to the attitudinal approach borrowed from Snyder and Diesing), we are left with no real clues, other than a vague notion about the interplay of institutional interest and basic attitudinal orientation, of why such issue groups emerge.

The absence of broader theoretical concerns aside, Bialer's edited volume still stands as the most complete discussion of the domestic inputs into Soviet foreign policy; we know more about the pieces of the puzzle, even though we are still unsure how to arrange them.

DONALD R. KELLEY

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The Local Politics of Rural Development: Peasant and Party-State in Zambia. By Michael Bratton. (Hanover, N.H.: University Press of New England, 1981. Pp. xii + 334. $17.50.)

Michael Bratton's book is part of a growing literature on local African politics, which avoids both structural-functional and Marxist paradigms in an attempt to explain the workings of subnational political and governmental structures in their diverse national settings. This particular study examines Zambia's Kasama District, and addresses three major questions to which earlier "nation-building" and more recent "center-periphery" analyses have provided some rather facile, and frequently inaccurate, answers. In his investigation of Kasama in the middle 1970s, Bratton asks how far into the district did the Zambian party-state actually penetrate; who benefited and who suffered in the relationship between the political and developmental center and its local periphery; and what mixture of local participation and central control resulted from the politics of rural development in this northern portion of Zambia.

Combining institutional and policy analysis, Bratton focuses his attention on rural party-government structures and on a postcolonial program of village resettlement which formed the core of Zambian development policy in the decade from 1964. The Zambian government and United National Independence Party (UNIP) are highly structured at the village, ward, district, provincial, and national levels. Bratton begins his assessment with a close look at the two most important local government structures, the Village Productivity Committees (VPCs) and the Ward Development Committees (WDCs).

The VPCs were intended to help implement national policy in the villages, and to encourage self-help modernization activities within the rural population. In a survey of 44 of Kasama's 496 VPCs, Bratton determined that fewer than 10 percent were active in fulfilling these responsibilities. He also found that both active and inactive VPCs had been "captured" by traditional leaders, who used the meager developmental resources supplied by the state as patronage to enhance their own positions of wealth, status, and power. The central government was compelled to rely on traditional leaders to promote such important policies as those encouraging the formation of larger village communities. But while larger communities were more receptive than smaller villages to developmental innovations, the generally dismal performance of the VPCs "showed chiefs and headmen to be disinclined to promote changes that undermined their own positions of authority" (p. 80) and access to patrimonial rewards.

A somewhat different pattern of control and participation appeared at the ward level, where traditional authority figures gave way to a leadership group representing salaried government employees, successful cash-crop farmers, and members of the local party elite. The Ward Development Committees were designed to provide social infrastructural services to the villages, engage in financial and development planning, and adapt central agricultural policy to local conditions. Bratton discovered that 12 of Kasama's 23 WDCs were active in performing these functions, but that their impact on local development was minimal. Major projects were devised and funded at the center; and although the WDCs exercised control over some revenues, these were few in comparison with centrally administered funds. Most important, the WDCs were used as patronage dispensing agencies by a self-serving group of local civil servants, party officials, and economically emergent farmers. Poorer peasants looked to the WDCs for a share of the national wealth; but with the central and local resources at their command, ward-level patrons "managed and constrained peasant demands on behalf of the center through the medium of the party-state apparatus, of which Ward Development Committees became the lowest echelon" (p. 122).

To shed additional light on the politics of rural development, Bratton examines the policy of village regrouping, which became the key element of Zambian rural development efforts in the 1960s and early 1970s. As elsewhere in Africa, the villagization of scattered rural populations was supposed to rationalize the delivery of central services to the localities, create centers of rural economic growth, stem the tide of urban migration, and provide examples of successful development to neighboring communities. Also in common with the experiences of other African countries, these goals were never achieved in Zambia.

Bratton presents case studies of two regrouping schemes, Nseluka and Kayambi. In his descriptions of these projects, he conveys many of the same kinds of problems that Tanzania experienced in its massive resettlement of fully 90 percent of its rural population. Peasants were generally willing to be moved, but only in return for "social services associated with an urban life style" (p. 187) and not because of their desire to engage in the hard work of modernization. Where some of these amenities were available, as at Nseluka, the villages appeared to prosper; and where they were absent, as at Kayambi, the program obviously failed. In neither scheme were local self-reliance and economic growth significantly enhanced, nor did these projects reverse the flow of urban migration from Kasama and prevent the further growth of an economically stratified rural society. The patronage system profited from village regrouping, as what developmental inputs were received found their way into the hands of a few relatively wealthy residents of the resettlement areas.

Bratton concludes his study with a penetrating analysis of the local party organization of Kasama. The changing fortunes of the United National Independence party confirm the patrimonial tendencies of Zambian local politics. UNIP gradually transformed itself from a loosely organized independence movement into a patronage organization whose main function became that of distributing rewards as a way of reinforcing state control over the countryside. By operating in this manner, the party also reinforced "a general process of stratification of peasant society between those who had benefitted from state resources and those who had not" (p. 266).

On the basis of his impressive array of data, Bratton concludes that the Zambian party-government maintains a fragile linkage with its rural periphery which is fed by selective and intermittent injections of patronage. The resulting relationships create a widening gap between the beneficiaries of this patronage and the vast majority of nonrecipients. This situation is not entirely one-sided. Excluded from the local reward system, the peasants are also able to escape the control of a regime which must trade material rewards for increments of compliance: "Below the level of WDC and party constituency little patronage permeated and little central control was enjoyed" (p. 274). As Hyden has recently confirmed in a study of rural Tanzania, this state of affairs does not bode well for the political future of any emerging African state. (See Goran Hyden, *Beyond Ujamaa in Tanzania: Underdevelopment and an Uncaptured Peasantry* [Berkeley: University of California Press, 1980]).

Bratton has provided us with a lucid study of political reality in contemporary local Africa. Rich in detail and empirically grounded analysis, the book should be widely read by serious students of Africa and by others concerned with the problems of discordant political change.

RODGER YEAGER

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Mexico's Leaders: Their Education and Recruitment. By Roderic A. Camp. (Tucson: University of Arizona Press, 1980. Pp. xvi + 259. $28.50, cloth; $12.50, paper.)

Roderic Camp's study of Mexico's top-level political leadership provides fresh perspectives on key dimensions of Mexican politics, such as personalism, trust, political cliques (*camarillas*), family loyalties, continuism and cooptation. The author's principal theoretical focus centers around three basic categories of recruitment processes: "(1) channels or routes to the top most commonly used by aspiring political leaders; (2) sponsors or selectors of those who reach top political positions, and the means or certification they use to screen prospective leaders; and (3) credentials or qualifications of the aspirants" (p. 4). Educational background and/or career preparation are the key concerns for data collection.

Roderic Camp has compiled an impressively extensive data base, especially so because of the difficulty in obtaining biographical material on the top-level political leaders of Mexico. In light of the problems previous researchers have consistently noted in gathering such data, the author's work, representing more than a decade of effort, is noteworthy not only as a base for his present work but also for the potential it holds for future scholarship. This is no sampling effort; the author attempts total coverage and is forthright in presenting successes and failures in light of his goal.

Two data collection projects were undertaken —(1) the Mexican Political Biography Project (MPBP) data for which the author believes his data about 75 percent complete and (2) an Educational Data Bank (EDB)—data that he believes encompass all individuals in Mexico's student population who became high-level officeholders after 1935. The MPBP data was the basis of a preceding volume, *Mexican Political Biographies: 1935-1975* (Arizona, 1976). By the time of the more recent publication, the MPBP data included 1,083 individuals politically active since 1935. Data was collected for 59 variables, and the individuals were grouped in three overlapping categories: "A general political elite"; "a government political elite"; and "an educational elite" (p. 231). Initially, over 900 persons politically active in the 1935-75 period were selected from known primary and secondary sources. Six hundred were known to be alive in 1971. Accurate addresses for 300 persons were available and interview/correspondence with them possible. Over 50 percent of these replies to an initial letter. Detailed correspondence and interviews were obtained for over 100 persons. It was this personalized effort by the author which made the whole project so exhaustive of time and funds but which yielded a generally more extensive and precise quality of information on a wide range of kinship ties, socioeconomic background and personal political alliances.

Camp's data show that higher education and career training have become increasingly important in recruitment. Linkages between the bureaucracy, the university, and the highest levels of political power have been steadily overshadowing recruitment from other institutional frameworks, e.g., the military, peasant and labor organizations and the official party (PRI) apparatus. The effect of regional ties upon preparatory and university educational backgrounds can be significant, and it is clear that Mexico City and National University outweigh the educational institutions of other regions, but certain other factors associated with the political culture can modify the relationship of recruitment and educational/career preparation. For example, "trust" is much sought after, seldom given, and almost never attained. Typically the individual Mexican expects to find very few persons that can be counted on for either benefaction or support. Thus evaluation of an individual's dependability and strength outweighs consideration of the procedural and publicly institutionalized recruitment channels. The keys to honest and desirable alliances are especially family ties, childhood friendships, shared career preparation and attachment to father surrogates—a university professor or a sympathetic elder in politics. The system encourages formation of cliques of intimates (*camarillas*) who enter politics together and intertwine in primary and secondary relationships revolving around leaders whose careers depend upon interpersonal ties. Both key leadership figures and their chief collaborators come to regard each other as nearly indispensable. Continuism in key places is a familiar pattern and is reinforced by cooptation of others who show political talent and who are converted from possible opponents to collaborators in the *camarilla*. Practices that might elsewhere be singled out as forms of nepotism are accepted more often than criticized. "Bureaucratic families," those with numerous members in public office, blend easily into the political scene.

Growing homogeneity of the political elite seems to be an emergent factor. Education, especially university education, while not an "exclusive influence over recruitment . . . does have a more substantial role than any other *single* institution or organization in Mexico" (p. 164). This trend is greatly reinforced by the recruitment roles played by key university professors (pp. 130-33). The importance of the university places in bold relief the schism between the offspring of business executives who traditionally are privately educated and students from the "political" families who traditionally go to government-supported universities. The differences in educational backgrounds invite speculation about the exacerbation of political conflict for the future. In addition to the universities' tendency to separate political and busines elites there is a similar divisive pattern in the educational background and career training of political aspirants and the new generations of military officers. This helps explain "why the military no longer serves as prime recruiter for men with political ambitions, nor does it provide an alternative route. . . . Mexico's political leadership . . . has increasingly lost contact with [the military's] leadership and has made the military's activities less visible" (p. 57). Interestingly enough, the author speculates along with Evelyn Stevens that "invisibility" of the military should not be interpreted as an automatic guarantee of military nonintervention in the future.

In summary, this volume is essential reading for the Mexicanists and Latin Americanists in the social sciences. It is also useful for comparative political analysis in its treatment of education in the recruitment process. It is, moreover, a major contribution which can substantially serve policy makers in the 1980s. Finally, it is a fine example of the value of a young author who contributes well-reasoned conclusions on the basis of his own extensive and seasoned data.

L. VINCENT PADGETT

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The Politics of Elite Culture: Explorations in the Dramaturgy of Power in a Modern African Society. By Abner Cohen. (Berkeley: University of California Press, 1981. Pp. xxii + 257. $20.00, cloth; $6.95, paper.)

In recent years, political scientists have shown a good deal of interest in the symbolic aspects of political life. Edelman, Nimmo, Cobb and Elder, and others have all written about the ways in which the organization and manipulation of political myths and symbols is crucial in enabling elites to maintain power and control. Anthropologists, such as Levi-Strauss or Turner, in contrast, have devoted more attention to the structure of symbolic systems, and have been less interested in linking them to social or political action. The work of Abner Cohen, a British social anthropologist, is an important exception. In an earlier study of the Hausa living in the Yorouba city of Ibandan, *Custom and Politics in Urban Africa,* and in a more general essay, *Two Dimensional Man,* he argued that the organization of the culture system is intimately related, but not reducible to, questions of political power and control.

In this superb new study Cohen develops more fully some of his earlier ideas showing how political elites act in terms of universalistic values in their public roles and particularistic ones as individuals at the same time. In a sense it is similar to Lasswell's concerns with private motives and public power at the social, rather than the psychological, level. Cohen asks what is the connection between the ways in which public power is organized in a society and the culture of the political elite. His careful analysis helps us understand how culture develops as a means for the coordination of the elite's activities and assists it in enhancing and maintaining power (p. 126).

This book is an important elaboration of the argument Mills presented in *The Power Elite,* that the rulers of society are not only linked through common interests in public problems, but also through shared culture. What is provided here is a well-developed case study, as well as a more general argument, concerning what those shared cultural interests are, how they develop and how they are maintained. While some of the details of Cohen's analysis of the Creoles of Sierra Leone, may seem exotic, his point is just the opposite. Rather he argues that the framework, developed from the study of the Creoles, a group which comprises only 2 percent of the population but dominates the upper reaches of the nation's civil service and judiciary, provides a basis for the comparative study of political elites.

The Creoles of Sierra Leone have faced a fundamental political dilemma since the end of the colonial era. As a small minority they cannot organize politically to achieve their particularistic goals. Instead they have developed a variety of cultural institutions which are not inconsistent with the more broadly universalistic values of the nation and their prominent role in its administration to serve their narrower needs as well. Cohen describes in rich detail, for example, the rise in Masonic Lodges and the increased importance of the cult of the dead among the Creoles. Here, as in many other cases where elites cannot organize formally, cultural institutions assist the elite in informally closing its boundaries within a formally open system, in developing a network of moral relationships to fulfill utilitarian ends, in practicing sponsored recruitment under the principle of equal opportunity, and in developing face-to-face communal relationships within large-scale organization.

One of the things this carefully organized, detailed analysis shows is that elite coordination is neither particularly simple to achieve nor necessarily a conscious process. Rather, the power of the rich ceremonial life of the Creoles produces a multiplicity of emotional reactions both within and outside the community. The importance of these reactions is that they help answer fundamental questions arising from the contradictions of the political and social order. "Elite culture can thus be seen as resolving, and in the process mystifying, these basic contradictions in elite organization. The process is partly ideological, partly dramaturgical; partly collective, partly individual, operating at different levels and at different places, in complex chains of sociocultural links" (p. 220). Symbolic processes which serve utilitarian functions also operate on the moral level.

While Cohen's analysis is developed on the social level, as we might expect from a British social anthropologist, Cohen does suggest ways of understanding individual mechanisms involved in the process. In particular, he shows the widespread participation in Creole ceremonial life (e.g., weddings or funerals are frequently attended by over 10 percent of the adult Creole population) and the importance of this for individual participants caught between particular wants and universal demands. Second, he points to the socialization process in inculcating the appropriate values in successive generations. There is a consistency between the values Creoles stress in the family, in the schools, in churches and in public life which both serve to integrate Creole culture and to maintain their positions of dominance in the civil service. Cohen mentions, more than he analyzes socialization, and it would be interesting to examine individual-level propositions concerning such concepts as identification which seem essential to his argument, but are not studied directly.

This is probably the most insightful book on political elites since the work of Mills. Its analysis goes a good deal beyond what political scientists traditionally study in this area pointing to a richer understanding of elite coordination and perpetuation. Culture from this perspective is a product of power relations in a society, but it is also dialectically related to the organization of power, so it also reinforces these relations as well. The framework Cohen proposes for extending this analysis cross-culturally would be one well worth taking seriously.

MARC HOWARD ROSS

Bryn Mawr College

The Rule of Law: Albert Venn Dicey, Victorian Jurist. By Richard A. Cosgrove. (Chapel Hill: University of North Carolina Press, 1980. Pp. iv + 319. $19.50.)

A. V. Dicey, at Richard Cosgrove's hand, emerges as the familiar constitutional authority, one of the more distinguished late representatives of the Benthamite-Austinian tradition. Dicey is also drawn as a refractory critic of democracy and social reform, whose political jeremiads, devoid of the balanced judgment of his legal studies, were driven both by ideological dogmatism and frustrated political ambition. Cosgrove's interest centers on these ideological enthusiasms; Dicey's constitutional analysis, while lauded, is more perfunctorily treated. Cosgrove's presentation of Dicey's legal scholarship, especially the central concepts of sovereignty and the rule of law, hinges on Dicey's own work and the reaction of later commentators, especially Ivor Jennings. The relevant work of Dicey's contemporaries and his historical antecedents are inadequately developed. The results are an exaggerated sense of Dicey's originality and the minimization of the difficulties of his position. It was not only later commentators who found fault with Dicey's fundamental distinction between legal (parliamentary) and political (electoral) sovereignty. Henry Sidgwick commented:

> The new view of a double sovereign differs essentially from the traditional doctrine: . . . and the convenience of using the same term to express two such very different kinds of power seems to me highly dubious (*Elements of Politics,* 2nd ed., p. 658).

Despite Cosgrove's endorsement—"in every sense Dicey provided the classic exegesis of sovereignty"—it is not self-evident that Dicey left a more viable concept than he had received; certainly Cosgrove does not make a convincing case for this claim. More fundamental criticisms by the pluralists are not examined by Cosgrove.

Cosgrove presents Dicey's rule of law concept as a contribution of considerable originality. True, but had it been placed in the context of continental antecedents, additional qualification would have become evident. Cosgrove says the appropriate things about the muddle Dicey made of *droit administratif,* a failure which, in its broadest implications, compromised Dicey's efforts to enhance the rule of law.

Cosgrove makes a persuasive case for Dicey's remarkably rigid conservatism. Although early in his career Dicey could be included among those academic liberals of the 1860s who welcomed working-class enfranchisement, he experienced an ideological metamorphosis by the 1880s, rejecting all but token democracy, denouncing the mildest forms of state economic intervention and the growing trade-union movement. Earlier he had justified democracy, in part, as a necessary concession to class hostility; in the end all concessionary politics were rejected in favor of the state's police power as the official instrument of social order.

This hardening of position reflects concerns Dicey shared with many of his peers, although his analysis is a rather inferior specimen of the genre. His is a world of disjunctions: without apology to the subtleties of British economics and political philosophy, Dicey provides political actors into the virtuous few and the vicious many. Cosgrove could have made more of this material, softening Dicey's ideological intemperance; however, the primitivism is essentially of Dicey's own doing.

To a considerable extent this extraordinary and surprising turn of a fine legal mind, as Cosgrove ably demonstrates, is to be understood as springing from Dicey's anguish over the Irish Question. An unrelenting Unionist, he would grant no honor or respect to any advocate of home rule, or the voters who supported them.

Cosgrove rings the changes on the consequences of the Irish Question on Dicey's attitude toward the enfranchisement of workers and women, the role of parties, etc. and touches on the Question's chilling effect upon British liberalism after 1886.

This study also stresses the prominence of ethical nationalism among Dicey's values. He was committed to a militant form of moral nationalism embodied in the state. All associational or other loyalties were subordinated to this overarching national goal. Cosgrove quotes Dicey in 1917: "The fine spirit of our armies is proof that the corruption of peace may go less deep than one sometimes has feared and that there are more heroes living around than are supposed" (p. 277).

Given this gulf between himself and classical liberalism, one finds bitter irony in Dicey's late defense of the reputations of Bentham and Smith from the criticisms of certain social reformers; his was the greater apostasy.

WILLIAM A. RITCHIE

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Soviet Regional Policy: A Quantitative Inquiry into the Social and Political Development of the Soviet Republics. By Jan Åke Dellenbrant. (Atlantic Highlands, N.J.: Humanities Press, 1981. $30.00.)

Soviet Regional Policy, a book of 138 pages, plus references and appendix, may be divided into three parts: a concise overview of theories of the Soviet system (pp. 1-18); a discussion of the research design and description of the behavior of selected socioeconomic variables for each of the 15 Soviet republics, 1956-1973 (pp. 19-100, 118-35); and a brief treatment of the theoretical implications of variations among Soviet regions (pp. 10-17, 136-38). The author's central hypotheses are: (1) "During the last two decades, the levels of modernization have been increasing in the Soviet Union" (p. 45); (2) "The geographical distribution of modernization is characterized by increased equality between regions" (p. 45); and (3) "The greater the degree of social mobilization, the higher the level of political recruitment [defined as total membership of party or youth organization]" (p. 47). Regarding the first hypothesis, the author, not surprisingly, finds growth over the period. Infant morality (evidence of deteriorating health-care delivery systems), much discussed now, is not a variable. The second hypothesis, he does not confirm, finding the same "level of differences" in 1956 and 1973, but "the differences certainly have not increased" (p. 102). This point requires a more finely tuned study of the dimensions of inequality. As Donna Bahry and Carol Nechemias have pointed out in their forthcoming article ("Half Full or Half Empty? The Debate over Soviet Regional Equality," *Slavic Review* 40 [1981]): "The shorter the period, the less likely we are to come up with a positive assessment." The choice of a period in which to test the central policy is crucial to the results. A baseline of relative equality yields only modest change toward equalization. Then, too, the impact of major policies, from initiation to implementation, depends on many durable and relatively intractable regional factors, particularly across an area as vast and historically and ethnically diverse as the Soviet Union. Even the definition of equality is problematical: is the pattern one of "compensation"—funding the less-developed areas at higher levels than the more developed, "equal appropriations," or "redistribution"—subsidizing the less developed, so that even though they spend less than others, they spend more than their level of development would predict? Dellenbrant's time frame and methods do not permit a satisfactory examination of these questions.

In part, the problem is one of data. Dellenbrant relies almost exclusively on centrally published statistical collections. Clearly, additional republic-level indicators would be useful. Regional handbooks are published in the Soviet Union. Dellenbrant cites only six of them. He would be able to use them systematically by working in large research collections. For example, in 1980 the Foreign Demographic Analysis Division of the U.S. Bureau of the Census had in its research library for specialists 260 general republic handbooks (92 percent of the total number published between 1956 and 1979). More difficult is gaining access to republic-level data sources for party membership variables. Here, too, there are a number, though not many, of republic- and province-level collections which would permit one to draw finer distinctions about the process of political recruitment and the association between that variable and the process of modernization. Undifferentiated aggregate membership statistics could thus be enhanced if one looked at social class, education, sex, ethnic identification, occupation, and other variables.

Analyzing the third hypothesis, Dellenbrant formulates a regression model in which political recruitment (membership of Communist party or youth organization) is the dependent variable and five variables representing components of modernization (urban population, scientific workers, industrial/workers index, physicians, newspaper publication) are independent variables. By extracting the three deviant cases (the Baltic republics), he confirms this hypothesis for 12 republics (R^2 = 80-95 percent across the years taken individually), with education the single more powerful predictor. However, Dellenbrant notes that the fit is less good toward the later years of his data set: "The reason for the more uncertain results for the latter part of the time-series seems to be that the increase in Party recruitment stagnated somewhat" (p. 114). This finding raises the interesting question as to how dependent the dependent variable is. The party is not a mass, but an elite, party; its history is one of expansions and contractions—a sequence of differing central recruitment policies. How those policies are implemented—on what dimensions and with what time lag—it itself an important theoretical and empirical issue.

Dellenbrant has provided a useful source and developed interesting patterns of development. It is to be hoped that he will expand and deepen his analysis of these important and difficult questions.

ELLEN MICKIEWICZ

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The State Tradition in Western Europe: A Study of an Idea and an Institution. By Kenneth H. F. Dyson. (New York: Oxford University Press, 1980. Pp. viii + 310. $19.95.)

"Besides referring to an entity or actor in the arena of international politics, state is a highly generalizing, integrating and legitimating concept that identifies the leading values of the political community with reference to which authority is to be exercised" (p. 206). Two hundred pages into a dense discussion, Kenneth Dyson wheels out a purported definition of the state which begins thus, and rolls on for another 80 words. The definition epitomizes his book in all respects but two: it omits his usual catalog of relevant thinkers, and leaves out his customary sequence of pertinent concepts, rendered in their original French or German. The definition's rush past the "entity or actor" to the ideas and values of leading thinkers, its use of weak verbs and passive voice to avoid attributing actions to agents, its abstractness and its excessive length all characterize *The State Tradition in Western Europe.*

It takes a while to discover the true character of this book. The outline of chapters, for example, is deceptively straightforward: the origins of the state, the state as a sociocultural phenomenon, the state and the intellectuals, three conceptions of the state (might, law, and legitimacy), the historical development of theories of the state, a comparison of ideas of the state in France and Germany, the word "state" in the British intellectual tradition, state as a tool of analysis, the idea of the state and democracy. Not a tight logical progression, to be sure, but a reasonable set of lecture topics. However, this promising outline fails to shape an effective book for several reasons: (1) because every discussion of the actual operation of particular states—even the discussions of origins and of sociocultural characteristics—rapidly veers into a discussion of ideas *about* the state in general; (2) because Dyson uses his comparisons chiefly to bring out peculiarities of one intellectual tradition or another, not to identify their common properties or their structural correlates; (3) because the abundance of Dyson's illustrations and asides hides his main points rather than clarifying them; (4) because the argument does not advance from one chapter to the next. Indeed, the chapters are nearly independent of one another.

Dyson seeks to prepare for his readers a "map-and-compass guide" to ideas of the state and attempts to apply those ideas in Germany, France, Italy, Britain, and other countries, in rougly that descending order. As a specialist in German social thought, Dyson is most comfortable with that tradition of thinking about the state, and is inclined to use it as a standard for the evaluation of other traditions. His self-appointed task is descriptive, and for the most part he proceeds very descriptively: reviewing alternative concepts and approaches, marching the thinkers past, enumerating difficulties and complexities, but not building a sustained argument. Dyson narrows the task, furthermore, by adopting a cramped view of "thinkers" and "thought": he ignores pamphleteers and orators, eschews the symbolic content of popular collective action, disdains analyses of implicit but pervasive conceptions of the state as revealed by ceremonies, iconography, judicial proceedings or folklore. The rich treatments of the state tradition by such authors as John Brewer, E. P. Thompson, Alf Ludtke, Simonetta Soldani, and Maurice Agulhon therefore find no place in *The State Tradition in Western Europe.* Only the sober, self-conscious theorists of the state appear. That choice increases the prominence of German thinkers and German ideas.

To the extent that any ideas recur throughout the book, they assert (1) the greater richness and value of concepts of the state in continental Europe than in the Anglo-American world; (2) the partial autonomy and influence of such ideas with respect to practical statecraft; (3) the compatibility—perhaps even mutual reinforcement—of democracy and strong state traditions. Dyson's dense description blocks any effort to present and then defend one or another of these arguments. The most systematic part of the book—a taxonomy of assumptions underlying alternative institutional arrangements—is a chapter adapted from a previously published article which remains quite segregated from the rest of the volume.

In his opening pages, Dyson holds out the hope that he will place crucial concepts in their exact historical settings in the manner of John Pocock or Quentin Skinner. That hope comes to naught; Dyson's penchant for stringing together many theorists and concepts and his zeal to establish the autonomy and primacy of ideas combine to defeat it soundly. Weber and Schmoller, *Machtstaat* and *Sozialstaat,* appear repeatedly in the discussion, but so fleetingly and in such numerous, varied company that the uninformed reader never has a chance to single them out and get them into focus.

For what profitable purpose, then, might we read this book? No doubt for its asides. There is the suggestion that world-saving conceptions of the state come into being in response to the visible threat of disintegration (p. 83). There is the insight into the vulnerability of British idealism, which failed to develop its own sociology and lacked resonance with the standard categories of British intellectual culture (p. 192). There is the account of Ralph Miliband's offense both to his enemies and to his ostensible allies. For these and other quick flashes of light, a thoughtful, tolerant reader has something to gain from Dyson.

CHARLES TILLY

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Paris and the Provinces: The Politics of Local Government Reform in France. By Peter Alexis Gourevitch. (Berkeley, Calif.: University of California Press, 1981. Pp. xiii + 250. $18.50.)

Paris and the Provinces examines reforms of center-local relations in France, inquiring into reasons for their ineffectiveness. Peter Gourevitch states that his principal goal is to "demonstrate the way policy toward the areal distribution of powers in France . . . has been shaped" by various forces and concerns (p. 4). He traces images of the diverse political families relative to centralized and unitary areal distribution of powers, changes in those images, and among each of these families the emergence after World War II of new criticisms of the Jacobin model. He concludes that the systemic cause of the blockage of central-local relations and the absence of real decentralization in France is fear of the left, particularly the Communist party. He notes that a new model of the politico-administrative system has not been produced, and he states, "To the extent that the attainment of such ends depends on party relationships, we are in for a very long wait" (p. 237).

Excluded from this study is treatment of the economic dimension of local government reform; the author tells his readers that "there is an intervening type of analysis required as well: the concerns of politicians and parties who have their own outlook and interests. And in their evaluation of risks and rewards, these politicians, as well as administrations, interest-group leaders, and the public are affected by ideological traditions concerning the distribution of authority" (p. 235). Where socioeconomic theorists focus on economic interests, Gourevitch prefers to focus instead on "partisan political stakes" (p. 237). Consequently, students of French politics need not search here for information relative to financial relations between the state and communes. The transformation of communal administration —a fundamental aspect of *planification*—is not developed. The very system that obligates the communes to increase continually local taxes goes untreated, and scant attention is extended to their permanent indebtedness. Finally, there is no concern here for the rising cost of services and construction that contribute to the financial crisis of the communes, denying them the opportunity to hold their own or ameliorate their indebtedness; (2) or to the decrease in state subsidies to the communes, limiting in turn new communal investments; (3) or to the necessity of obtaining communal loans for the financing of local equipment expenses, the interest charged, and the financing of these loans by newly levied local taxes; (4) or to the modernization of local taxes.

Considering that Gourevitch's purpose is to "demonstrate the way policy toward the areal distribution of powers in France . . . has been shaped by various forces and concerns," it is difficult to understand why he chooses not to deal with those fiefs with local systems and local strategies that have little to do with national cleavages and conflicts, ones that rest generally on tolerance by the national system. Ignored, for example, is Bordeaux, the fief of Jacques Chaban-Delmas, "the duke of Acquitaine," and a mode of operation that depended for years on a Parisian career and the backing of local elements, both Gaullist and Old Left. For years few elections were contested, few incumbents were defeated, and the status quo was effectively consolidated by Chaban-Delmas. Chaban-Delmas in fact contributed in 1965 (not visibly) to the election of M. Deschamps, Socialist mayor of Talence, and subsequently to his appointment as Vice-President of the Urban Community of Bordeaux. This local fief was dominated for years not by fear of the left but by recognition of provincial realities, despite the incompatibility between local and national exigencies. Called to the reader's attention by the reviewer are the following works on Chaban-

Delmas: Jean-Francois Médard, "Les structures politico-administratives," *Revue Géographique des pyrénées et du Sud-ouest*, Tome 42, Fasc. 4 (1971), pp. 411-30; Jean-Francois Médard, "Clientelisme politique et pouvoir local en France (note introductive)," *Association francaise de science politique*, unpublished (1973), p. 17; Jacques Lagroye, "Milieux sociaux et vie politique: une étude de cas, le député maire de Bordeaux 1946-65," *Thèse 3e cycle*, Bordeaux, 1973. Finally, Gourevitch ignores other local fiefs also deserving of attention.

Paris and the Provinces assesses on a limited scale a problem that merits serious attention. Perhaps Gourevitch will choose to address himself in the future to other and broader aspects of French decentralization. His present study constitutes an intriguing beginning.

LOWELL G. NOONAN

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Britain: Progress and Decline. Edited by William B. Gwyn and Richard Rose. (New Orleans: Tulane Studies in Political Science, 1980. Pp. xiii + 164. $17.50.)

Of the contributors to this book, seven are political scientists, and one is an economist. Since the connection between economics and politics is important to this volume, one might lament the absence of more economists, but in fact the best description of the British economy is given by a political scientist, Graham Wootton. More serious is the absence of sociologists, cultural anthropologists, and especially—given the longevity of the British political system—historians. Lack of historical perspective weakens the book's contribution to an understanding of political change in Britain. One author mentions the significance of the past (p. 57), but does nothing with it. For another (p. 146), 1936 is "long ago," and a third constructs a typology of prime ministers including Joseph Chamberlain and Hugh Gaitskell (pp. 74-75).

The editors give as their two goals to describe and to evaluate "the major changes that have been taking place under the auspices of British government since the creation of the mixed economy welfare state . . . in the 1940s" (p. vii). Much of the nationalization of industry surely took place in the 1940s, but what about the impact of that nationalization? That the welfare state was created in the 1940s is debatable. Hugh Heclo's interesting discussion of welfare in Britain does not mention the name of David Lloyd George, whose 1909 budget shook Britain to the core, giving hope to the poor and fear to the rich. How can one understand the acceptance of the welfare state by the Conservative party after 1945 if one forgets that Lloyd George's chief ally in 1909 was Winston Churchill?

The editors' goals imply substantive analysis of government policies after 1945, but there is surprisingly little of that. Much of the discussion is of changes taking place—as in the social composition of the political elite, stressed by Dennis Kavanagh—independently of government auspices or even—as in the cases of inflation, balance-of-payments problems, violence in Ulster, and racial tensions—against the intentions of the British government. Much more, and much more clarity, is needed, even here, on the party system, the Commons, and the cabinet. These are still the major institutional components of the British political system, and one should not lose sight of the forest. Perhaps there is more stability in British politics than this book assumes?

To criticize a book subtitled *Progress and Decline* for insufficient attention to stability may be a bit unfair. It seems entirely fair, however, to observe that this subtitle, intended to give unity to the book, states a theme which is not significantly developed. The concept of progress remains undefined, and plays no real role in this book. The emphasis is on decline, but even this concept is not adequately defined or used consistently. Jeremiahs on the subject of decline are distinguished from pragmatists toward problems (p. 17). Optimism about British politics appears not to be an appropriate conceptual category for Gwyn and Rose. Failures, or at least what are assumed to be failures, are almost endlessly enumerated, but not successes. If the recent achievements of the British political system are indeed so minimal, why do so many Britishers express satisfaction (pp. 19, 25) with the quality of their lives? Could it be that they know something some professors do not? Could it be that, with the notable exception of Ulster, British politics is still as humane and decent as politics anywhere in the world? Could it be that the political system which so impressed the authors of *Congressional Government* and of *Towards a More Responsible Two-Party System* still lives?

MARVIN RINTALA

Boston College

Poverty and Revolution in Iran: The Migrant Poor, Urban Marginality and Politics. By Farhad Kazemi. (New York: New York University Press, 1981. Pp. 180. $17.50.)

Although the Iranian revolution of 1978-79 already has generated a score of books attempting to explain the causes for that political upheaval, the majority of these studies have failed to provide any insights due to a common inability to move beyond simplistic generalizations to more rigorous analyses of various segments of Iranian society in the 1970s. (See further my review article, "The Iranian Revolution," *Middle East Journal,* 34, No. 4: 484-89.) For this reason Farhad Kazemi's case study of rural-to-urban migrants in Tehran is a most welcome introduction to at least one of the major socioeconomic problems of Iran during the last years of the *ancien régime.* In addition, *Poverty and Revolution in Iran* is a significant contribution to the general field of comparative politics since it helps to foster an understanding of some of the consequences of rural-to-urban migration for Third World cities in general.

Kazemi's thesis is that the failure of the land reform program of the 1960s caused landless villagers and subsistence peasants to migrate to the cities in search of work. However, their poverty is perpetuated after migration since they "lead a marginal life on the fringes of urban society" (p. 4). The main characteristics of their marginality are inadequate income, substandard housing, political nonparticipation, and nonprivileged social status (p. 6). By the mid-1970s an estimated one million of these urban marginals had settled in Tehran. Kazemi distinguishes two groups among them upon the basis of residence and occupation: squatters who live in makeshift dwellings on property to which they have no legal title; and nonsquatting migrants who live in rented quarters in slum areas. The former "rarely have regular wage-earning jobs" (p. 3), while the latter tend to have steady employment. In general nonsquatting migrants tend to have more education, as well as better jobs, housing, and incomes.

Kazemi's most significant finding is that residence in Tehran did not increase political participation rates among the poor migrants. Before the revolution their political awareness was very low and they had virtually no involvement in political parties. Furthermore, migrants made few demands upon the system and avoided acts of political protest. For these reasons their mass participation in anti-regime activities during 1978-79 was unexpected. Kazemi believes that the change from political noninvolvement to political participation was a result of the successful mobilization efforts of the Shi'i Muslim clergy. The latter not only had widespread legitimacy as religious authorities, but also were popular because of their role as distributors of charitable aid in the slums, a widely appreciated welfare service which was in stark contrast to the government's policy of "benign neglect" (p. 114) of the poor.

The empirical evidence to support the above arguments was obtained from the quantitative analysis of responses to pretested questionnaires which were administered to randomly selected samples of poor migrants as part of Kazemi's field research in Tehran's slums during 1974-75 and 1977. The results are summarized in more than 25 tables and graphs. These data constitute an invaluable source of information about the socioeconomic conditions and political attitudes of rural-to-urban migrants, a group which heretofore has not been the subject of scholarly research. However, this book is not simply a case study of urban poverty in Iran. Throughout the text Kazemi draws upon his apparently extensive knowledge of the literature which deals with urban migrants in other regions, especially Latin America, in order to compare and contrast the situation in Tehran with other Third World cities. From this perspective *Poverty and Revolution in Iran* also provides useful insights into some of the political problems which can develop in countries which have large migrant populations living marginally on the fringes of urban society.

ERIC J. HOOGLUND

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A History of the Romanian Communist Party. By Robert R. King. (Stanford, Calif.: Hoover Institution Press, 1980). Pp. xvi + 190. $8.95, paper.)

Democratic Centralism in Romania: A Study of Local Communist Politics. By Daniel N. Nelson. (Boulder, Colo.: East European Monographs, distributed by Columbia University Press, 1980. Pp. xii + 186. $15.00.)

Both of the volumes under review are by political scientists, both focus on Romanian politics, and both are admirable attempts to describe and analyze important issues. Here the resemblance stops. The two works are totally different in goals, sources, and methodology. Robert King uses what might be termed an historical approach in his survey of the development of the Romanian Communist party (RCP), while Daniel Nelson poses and tests hypotheses applicable to comparative studies of political development. King's book is broader within the Romanian context, covering the evolution and institutionalization of the RCP over half a century. Nelson deals with specific issues of modernization/development during the 1960s and 1970s, but does so in such a way as to make his hypotheses and conclusions relevant outside of Romania and communist politics. Both authors are eminently qualified to write on Romanian politics, though for contrasting reasons.

Nelson has engaged in extensive field research within Romania, and his sources include interviews and questionnaires as well as research in Romanian libraries. King was for a number of years Senior Analyst for Romania and Assistant Director of Research at Radio Free Europe in Munich; this precluded visits to Romania, but gave him access to the research facilities at RFE, including all relevant Romanian publications.

Robert King has written a clear and concise survey of the development of the Romanian Communist party through the mid-1970s. Although his major interest is the institutionalization of the party itself, he summarizes the Romanian background as well, and so provides a useful text for courses in comparative communist politics. King's special strength is his knowledge of recent Romanian historiography. The RCP is constantly reinterpreting its own history; consequently, no history of the party has yet been written in Romania despite repeated promises. Instead, a number of shorter monographs and articles have appeared, and King's notes provide a valuable bibliography of these sources, as his text gives an excellent analysis of the reinterpretations. King views history as a political scientist: that is, his major interest is in the contemporary political process and what the study of history in Romania reveals about current politics. Indeed, his introduction is titled "History as Science, Myth, and Ideology." The chapters are arranged topically, but follow a roughly chronological order from the interwar legacy through the seizure of power to the institutionalization of the Romanian Communist party. Separate chapters then deal with the party elite, the relationship between party and society, the Romanian nation and nationalism, and foreign policy. King places Romanian developments in the context of comparative communist studies and summarizes quite effectively the debates among Western scholars on specific questions (the timing of the origins of Romania's autonomous foreign policy, for example). Unfortunately, with few exceptions (such as the author's own work on national minorities and foreign policy) the analyses and events cited all date from 1975 or earlier.

Daniel Nelson examines the local political process in Romania, specifically the relationship between socioeconomic and political change, in "an effort to apply fairly common assumptions about 'developing/modernizing' polities in the *non*-communist world to the case of an Eastern European state" (p. ix). This is an admirable goal; studies of communist politics have tended to focus on the pinnacle of the power structure, the top leaders, and to be country-bound or at most bloc-bound, limited to one state or to the boundaries of comparative communism. Nelson distinguishes between "development" (expanded social and resource exploitation, *what* a society accomplishes) and "modernization" (greater specialization, complexity, secularity, and participation, *how* a society accomplishes its goals). He also differentiates between *political development* (the ability to solve problems, to produce and cope with change) and *political modernization* (political specialization: mass parties, extensive bureaucracies, specialized executives). Nelson asserts that development and modernization usually accompany each other, but that the relationship is dialectical and therefore occurs unevenly and produces conflict. Romania, he argues, is "politically 'developed' " but "politically 'anti-modernization' " (pp. x, xi, 8-9). The RCP, he contends, deliberately slows socioeconomic modernization (but not development) in order to postpone political modernization.

It is impossible to do justice to Nelson's entire argument here; suffice it to say that, in order to test his dialectical hypothesis, he sought signs of conflict in local politics. In a series of interviews conducted in 1973, he studied four Romanian counties at varying levels of socioeconomic development/modernization, and found a positive correlation between rapidity of socioeconomic change on the one hand, and differences of opinions and backgrounds, competitive behavior, and role conflict among local officials on the other. Given the limits imposed on the interviews and questionnaires, Nelson is reluctant to come to a more positive conclusion than to suggest "the possibility that a dialectic approach might be a valid way to analyze politics in developing states" (p. 147). Perhaps the most interesting finding is the extensive disagreement among local officials "across Romania and within any single county as to what issues are most important and what are proper leadership roles" (p. 143). But perhaps the most valuable contribution of the volume is the picture Nelson gives of local politics in Romania: the backgrounds, functions, and concerns of the officials he interviewed, and the nature of the political process at this local level so frequently neglected in studies of communist politics. Unfortunately, Nelson's secondary sources date primarily from 1973 and earlier, and the many typographical errors detract from the quality of the publication.

MARY ELLEN FISCHER

Skidmore College

Who Were the Fascists?: Social Roots of European Fascism. Edited by Stein Ugelvik Larsen, Bernt Hagtvet and Jan Petter Myklebust. (Oslo, Norway: Universitetsforlaget, 1981. Pp. 816. $48.00.)

As is often the case with volumes of collected essays, the advertising blurb, and the editors' preface, make notoriously poor guides to the actual content of this tome. The blurb tells us that the collection will attempt to resolve the question of whether there was (or is) a generic phenomenon called "fascism," or whether what is spoken of as "fascism" was "a national response to specific problems arising out of and comprehensible only in the context of national historical trajectories?" The editors' preface further tells us that the aim of the volume "is to construct a theory of fascism which is explicitly comparative" (p. 9).

Actually, the very existence of the volume would seem to presuppose the answer to the first question. The volume is divided into the following parts: (1) On the Theoretical Study of Comparative Fascism, (2) Varieties of Fascism in Austria, (3) The Fascist Core Countries: Germany and Italy, (4) Fascism in Eastern Europe, (5) The Diffusion of Fasscism in Southern and Western Europe, (6) Fascism and National Socialism in the Nordic Countries, and (7) Comparing Fascist Movements. If the existence of a "generic fascism" were not assumed, the articles which constitute its substance could not have been collected.

We are left, however, with some misgivings. Part 6 makes a distinction between "Fascism" and "National Socialism." Stein Larsen writes of "fascism" in Norway, but Malene Djursaa remains content with "Nazism" in Denmark. Berndt Hagtvet informs us of Swedish "fascism," while Asgeir Gudmundsson refers to "Nazism" in Iceland. Bruce Pauley (in part 2) goes so far as to seemingly distinguish between "Nazis" and "Heimwehr Fascists" in Austria, only to retract the distinction by referring to "Austrian Nazis" as being "like all other Austrian fascists" (p. 231). Were Nazis "fascists" or not? Stanley Payne offers us not merely two, but six, varieties of "fascism" (pp. 21-22). We have a "paradigmatic Italian fascism" that is "not easily definable in simple terms." And then we have a "German National Socialism" that is not only "distinct," but which began to "develop its own system." Then we have a "marginal" Spanish Falangist type, a Romanian Legionary variety which is equally marginal, an Arrow Cross Movement, which is no more than "somewhat distinct," and finally there are "abortive undeveloped fascists" of all sorts. For his part, Alan Milward makes a valorous and instructive attempt to provide some notion of a generic "political economy of fascism" (pp. 56-65), but succeeds in talking almost exclusively about Fascist Italy and National Socialist Germany. There is no suggestion that anything of the same sort might be said of Romanian or Hungarian "fascisms," much less of their Irish or Portuguese variants.

We are really left to our own devices to try to fathom out in what sense all "fascisms" were the same, and in what sense they were distinct. Was the Austrian Heimwehr with its overwhelming peasant base, and its absence of ideology, really a member of a generic category, or a marginal variety, a deviant form, or simply a traditional conservative anticommunist movement? In fact, the question of whether or not there was (or is) a generic fascism is not answered in these 50 essays. Had the editors not discreetly chosen to limit themselves to Europe, the issue would have been twice confounded. Had they pursued some of the suggestions contained in the various essays and sought out the "fascisms" of Peròn's Argentina, Vargas' Brazil, Pinochet's Chile, and the ha'penny "fascisms" of North America (pp. 21, 44, 55, 155, 164, 169, 187), classificatory and definitional problems would have increased geometrically.

Because these analytic problems are not resolved (even in the context of Europe), any hope the authors may have had of affording a general "theory of fascism" is unrealistic. Since there is little that might pass as consistent pretheoretical classification, one could hardly expect explanatory theory to emerge.

The failure on the part of specialists to generate generally convincing theory seems to turn not only on the fact that coherent typological or classificatory schemata are not available, but on their failure to agree on critical descriptive facts as well. For example, Bernt Hagtvet refers confidently to the fact that the National Socialist German Workers party was a "movement of the middle class and the petty bourgeoisie," and that the "working-class was heavily under-represented" (p. 83). For Reinhard Kuehnl the party had a decidedly "lower-middle-class character" (p. 121). Peter Merkl, on the other hand, argues that "to picture Nazism as the revolt of a static German lower-middle-class . . . turns out to be largely myth and mystery" (p. 766). He goes on to indicate that "the percentage of working-class members of many fascist parties is not all that much lower than that of comparable socialist parties" (pp. 767-68). In effect, much of the data available has not received a standard interpretation. There is a spurious rigor in the quantitative estimates given for such vaguely characterized categories as "middle-class," "petty-bourgeois," "lower-middle-class," and "proletarian."

In substance, collections like *Who Were the*

Fascists? do not, and cannot, provide answers to the kinds of questions dear to the hearts of editors and those who put together blurbs on dust jackets. The wealth of such collections is found in the detailed treatment of various political movements identified by some vague set of criteria as "fascist." In such collections, for example, one can find a discussion of the Irish "Blue Shirts," and the Finnish People's Patriotic Movement—groupuscules that surfaced and disappeared in the vortex of European history between the wars. There is a great deal of pretheoretical speculation which is all interesting and often of heuristic merit. Juan Linz's assessment of the conditions facilitating fascist mobilization and political prevalence, and Zeev Sternhell's survey of fascist trends in France, are, themselves, worth the price of the volume. But one can hardly expect to find a generally acceptable social science theory that pretends to explain the rise, function and prospects of such an ill-defined class of complex historical movements. The problems that apparently prompted the crafting of the volume still remain.

The volume is to be recommended for what it is, rather than for what it aspired to be. It is a veritable encyclopedia of information about European political movements generally characterized as "fascist" during the interwar years. As such, it falls into the genre of books that have become so popular in our own time—that promise to deliver "All You Ever Wanted to Know About, . . . But Were Afraid to Ask." And like those books, this tome is a prodigious undertaking offering contributions of varying quality—but, all in all, a work that should be on the shelves of all serious institutions concerned with recent European history.

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Religion and Politics in Latin America: The Catholic Church in Venezuela and Colombia. By Daniel H. Levine. (Princeton, N.J.: Princeton University Press, 1981. Pp. xii + 342. $22.50, cloth; $6.95, paper.)

Following Kalman H.. Silvert and Leonard Reissman (*Education, Class, and Nation* [New York: Elsevier, 1976]), Daniel H. Levine contends that social science "must both produce its own synthesis and recognize the many creative syntheses invented in the normal course of social life" (p. 302). This he undertakes in his dissection of the Catholic church in Venezuela and Colombia while seeking illumination of the intricately complex interrelationship between religion and politics. With the former he considers questions of authority, visions of the Church, and experiential attitudes. For the latter, equally important factors include the transformation of types of politics, visions of the political, and comprehension of the nature of political experience. Adopting what is explicitly characterized as a phenomenological approach, Levine has produced a study which combines a deeply personalized concern for his subject matter with a markedly self-conscious orientation toward social science inquiry.

In the two introductory chapters and then again in his concluding "Further Reflections," Levine writes at great length about intellectual paradigms and analytic methods. Drawing on Geertz, Weber, and other observers of social science concepts and principles, he espouses the historical-institutional approach suggested by C. Wright Mills, thereby stressing substantive problems within particularistic historical reality. Throughout the work he repeatedly outlines, explains, and justifies the origins and structure of his work. The outcome, he hopes, will be "a more complete and accurate portrait of the processes under study than would be rendered by a more conventional, straight-line narrative approach" (p. 17).

The bulk of his field research was conducted from 1971 through 1973, and is heavily dependent upon structured interviews with 60 bishops, auxiliary bishops, and archbishops. He also incorporated interviews with leaders of national Catholic organizations and bureaucracies. In addition to necessary supplemental archival work, he singled out six dioceses—three in each country—for more intensive analysis. While the interview data are liberally sprinkled throughout much of the text, they appear in most concentrated fashion in the book's middle third. Employing both hard empirical data and a generous illustrative sampling of direct quotations, Levine has provided a rich trove of analytic gems and insights which cannot be adequately enumerated here. For scholars already immersed if the subject, substantial portions of these findings will have already been read in several articles published toward the close of the 1970s.

Comparisons between the Colombian and Venezuelan churches present few surprises for those familiar with the two countries. In reflecting the broader political character of each country, the analysis is clearly consistent with recent and contemporary patterns. At the same time, Levine effectively documents the breadth and range of diversity in structures, attitudes, and perceptions. He traces the forces which by the mid-1970s were leading Colombians to societal critiques placed within renewed emphases on traditional models of the Church. This contrasts with the the Venezuelan ecclesial style, more receptive to innovation and change, yet expressing a relatively more cautious social position. There is an enduring awareness of the respective societal settings within which events have been unfolding.

Having developed research interests in Venezuelan politics for his dissertation work and thereafter, Levine's choice of countries is equally predictable and sound. Among other considerations, it responds to the open and pluralistic political systems in Colombia and Venezuela, for which there are at this writing very few counterparts in Latin America. If comparative analysis it thereby enhanced, it nonetheless renders somewhat more difficult the effort to generalize for the entire region. If the book's title is perhaps less than a misnomer, the subtitle is more accurate. Indeed, the author is properly cautious with generalization. This is not to say that his work lacks relevance beyond the borders of the two countries in question.

Many passages bear consideration for those seeking an understanding of religion and politics in Latin America. There is a thoughtful treatment of the typologies of Church elites developed by Vallier and Sanders, accompanied by Levine's own formulations (pp. 131-41); a succinct review of Christian Democratic philosophy as stated by Maritain, Mounier, and Teilhard (pp.. 33-36), reiterated assessments at several junctures of Vatican II and the respective 1968 and 1979 General Conferences of Latin American Bishops in Medellín and Puebla; and the contrasting syntheses undertaken by exponents of the Theology of Liberation and by adherents of Societies for the Defense of Tradition, Family, and Property (pp. 41-51).

If *Religion and Politics in Latin America* is less than a seminal work, it nonetheless constitutes a solid contribution to a topic which has traditionally been marked by singularly unimaginative and pedestrian scholarship. The data are fresh, analysis is informed, and the conception of the work is thoughtful. A new generation of comparativists concentrating on religion and politics has recently emerged, promising increasingly fruitful scholarship, and Levine is among its more prominent members.

JOHN D. MARTZ

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Images of Development: Egyptian Engineers in Search of Industry. By Clement Henry Moore. (Cambridge, Mass.: MIT Press, 1980. Pp. x + 252. $25.00.)

The central argument of this book is that in Egypt the "Nasirite model"—an authoritarian-corporatist state following populist and etatist policies—proved incapable of leading to modernization. Nasir claimed to be carrying out a rationalizing-modernizing and egalitarian revolution from above; testing this claim through an empirical study of a group which played a strategic role in the enterprise—engineers—Clement Henry Moore finds that, owing to the authoritarian political system within which they had to operate, engineers contributed little either to rationalization or to equality.

Nasir's regime, Moore argues, degenerated into "Sultanic socialism," a system in which the leader, seeking to concentrate and personalize power in his own hands, stimulated rivalries among subordinates at the expense of unified elite direction of the task of development, and rested his rule on clientage networks which could neither mobilize support for modernization nor effectively implement modernizing policies. Thus the system, in its effort to take over and run the economy, crippled the market mechanism without being able to put a coherent and enforceable state plan in its place; unable to mobilize the masses, it could not prevent the elite from serving private interests at the expense of the public. Supporting this argument through case studies of several policy areas, Moore seeks to show that intraelite rivalries and politically motivated employment practices prevented enforcement of an education policy appropriate to Egypt's personnel needs; that elimination of private constituencies for scientific research in the absence of coherent planning prevented Egyptian science from maintaining research quality or adapting imported technology to Egypt's needs; that by denying engineers freedom of technical criticism and by overriding their recommendations on political grounds, the regime sacrificed the potential contribution of engineers to rational policy making; and that as managers and administrators their contributions were vitiated by clientelism. Finally, far from becoming a channel for enhanced equality of opportunity, engineering was dominated by the upper classes who, recruited into top roles, used their positions to further class and personal interests.

This book has many strengths. Moore's research strategy, testing a regime's performance in a few strategic policy areas, is a step forward in Middle Eastern studies, and the richness of his data, collected in an unfavorable research climate, is impressive. Digging below surface appearances, Moore provides suggestive insights into the dynamics of authoritarian-etatist regimes: how supposedly technical issues of development, having implications for interests, become foci of intraelite struggles which can paralyze even ostensibly "strong" states; and of how etatist models seeking to avoid the costs of both capitalist and communist systems, may deprive themselves of the strengths of either. Moore's data on the social background of engineers provides valuable confirmation of the extent to which the Egyptian upper class was able to adapt itself to the populist state and gives concrete meaning to the claim that Nasir's "socialism without socialists" was doomed. These are all important insights into Nasirism's failure to prevail.

On the negative side, Moore's conceptualization of Nasirism as technocracy derailed by neopatrimonialism, ignoring its charismatic populist dimension, is perhaps oversimple. His analysis, stressing exclusively the costs and failures of Nasirism, to the neglect of its admittedly flawed achievements—agrarian reform, the High Dam, reduced class gaps, the extraction of massive foreign aid from East and West, creation of an industrial base, checking a half-century of decline in living standards—will seem one-sided to many. Moore often seems so intent to make his case that he slights the significance of some of his own findings: he ignores evidence that technical competence played a role in recruitment since it fits poorly with the clientalist thesis; technocrats like Aziz Sidky are acknowledged to have had considerable power to direct modernization in their own domains, and a new breed of organization men are found to have emerged from the public industrial sector, but Moore still concludes that their contribution to modernization was a "hallucination." The attribution of Egypt's modernization failures to the political system slights alternative explanations—such as Egypt's high population growth and very scarce resources, defense burdens and the 1967 war—and ignores the fact that many of these failures are universal in the Third World, irrespective of type of political system. Finally, Moore is too incautious in drawing unqualified conclusions from often slim and ambiguous evidence, notably in attributing motives to elite actors on the basis of anecdotal evidence, and in inferences from sometimes weak statistical associations between dubious attitudinal indicators.

This is nevertheless an important contribution to the development literature, perhaps more provocative than balanced, but in spite of or because of this, a study no student of authoritarian-populist models can afford to ignore.

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The Backbench Diaries of Richard Crossman. Edited by Janet Morgan. (New York: Holmes & Meyer, 1981. Pp. 1136. $35.00.)

The Castle Diaries 1974-76. By Barbara Castle. (New York: Holmes & Meier, 1981. Pp. x + 788. $42.50.)

The Crossman volume edited by Janet Morgan should not be confused with the three volumes of Richard Crossman's diaries covering the period from 1964 to 1970 when he was a cabinet minister, nor with the one-volume condensation of those three. This volume covers the years when Labour was in Opposition from 1951 (when Crossman had been an MP for six years) to 1964. Morgan worked closely with Crossman on the earlier three volumes. Not only did she provide notes and explanatory linking passages, but also, since he died while the volumes were being prepared for publication, she really was the editor for the second and third ones.

Legal action by the British government alleging that Crossman had violated the confidentiality required of cabinet ministers delayed for many months publication of the first volume of the diaries. With readers' appetites thus whetted, expectations were high, but were not disappointed. The diaries have been widely praised—not for trivial *National Enquirer*-type gossip—but for the revealing and highly significant information they contain regarding how policy was formulated and implemented in the Labour party, the cabinet, and the bureaucracy. Our knowledge of cabinet procedures and power relations, of interaction between ministers and top civil servants has been greatly enhanced by Crossman's diaries.

Nonetheless, the appearance of the new volume raises the question as to whether we need it as well: do we need this record of the period when Crossman had no government responsibilities and was only gradually rising to prominence in the Labour party? The current state of the Labour party argues for an emphatic yes. Every generation Labour tries to tear itself apart. The period of the diaries saw first the Bevanites and then the unilateralists, with Crossman in the thick of both conflicts. Change a few names and labels from that period and you would think you were reading an entry for 1981. Thus the diaries help to provide a historical context in which to place the current factionalism. Nonetheless, Crossman's record exhibits significant gaps. William Rodgers, the chief architect of the unilateralists' defeat, gets only a single casual mention and not one word is said about the organizational vehicle for reversing the 1960 policy decision, the Campaign for Democratic Socialism.

Although Crossman originally intended the diaries to serve as raw material for a book on the British political system that he hoped to write, this volume has fewer of the reflections on power relations that made those published previously especially interesting. These broader concerns are not entirely absent, however. Given the extent to which Crossman's name is associated with the principle of prime ministerial government, it is rather surprising to discover that not until February, 1959 did Crossman come to realize that the House of Commons was, in Bagehot's terms, a dignified part of the constitution.

Almost overshadowing Crossman himself in these diaries is Nye Bevan. Despite the affection that he felt for Nye, Crossman's comments clearly reveal the indolence, indecisiveness, and irresponsibility that limited Bevan's effectiveness. The editor has removed potentially libelous material from the published version of the diaries, but strong judgments remain. Crossman's first impression of Henry Kissinger was of "a rather fat, oily, pompous but nice American Jew and so traitorous and anti-American that he even put me in the shade in the horrible things he said about his own country" (p. 751).

Janet Morgan appears to have done a good job in editing a mass of material into a comprehensible book with useful transitional passages and extensive footnotes. Unfortunately, however, she has an irritating tendency to explain the obvious; for example, detailed notes explaining references to *Das Kapital* and the "Red Flag." While this volume is not as important as the three previous ones, still it is generally up to their standard.

The Castle diaries—intended to be the first of a series starting, like Crossman's, with more recent events and working backwards—are much more chatty. While Crossman tried to separate the political from the purely personal and to record the former, Castle was less concerned to do so. As a result, we get more of an understanding of her as a person at the cost of a good bit of trivia. One rapidly tires of knowing how often she had her hair done and the reaction to the clothes she wore and of being told that she said something "sweetly." The air of self-justification pervading the diaries, largely absent from Crossman's, serves to confirm her confession that she frequently lacked self-confidence.

By the time of these diaries Castle was on her way out and at times seems to be on the periphery. One can learn more about how the cabinet made a deal with Chrysler to prevent its closing of British operations from the Granada TV program recreating the decision-making process than from these diaries. Naturally, her record does deal with the making and implementing of policy, but does not add much to the picture already provided by Crossman. It is interesting, however, to have yet another perspective on how Harold Wilson sought to manipulate the cabinet.

Barbara Castle, widely regarded as a true Bourbon of the left, provides evidence refuting those who believe that British politics has become ideological. She comments, "once again I realized that these right-wing and left-wing allegiances are due as much to the chemical reaction between people as to ideology" (p. 161). And never mind the commanding heights, "socialism isn't just militant trade unionism. It is the gentle society, in which every producer remembers he is a consumer too" (p. 309).

Castle is an interesting, prominent personality in Labour politics. Her diaries are significant as the record of the woman who, until Margaret Thatcher and, perhaps, Shirley Williams, had gone the farthest in British politics. Yet on the basis of this showing, those who want to learn about the workings of the British political system will find the Crossman diaries much more helpful and stimulating.

JORGEN S. RASMUSSEN

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Islam and Politics in East Africa: The Sufi Order in Tanzania. By August H. Nimtz, Jr. (Minneapolis: University of Minnesota Press, 1980. Pp. xvi + 243. $20.00.)

Islam today is perhaps the most significant monotheistic religious system to the politics of African societies. In general, however, its impact on politics is more evident in North and West than in East Africa. The explanation for this disparity is both historical and social/cultural. Though Islam spread in all areas of Africa almost simultaneously, its transformative effect was much more pronounced in societies of North and West Africa; hence the independent states of North and West Africa had to contend with that impact as they shaped their contemporary systems. By contrast, Islam's spread in East Africa and its impact on politics were much more variegated; it was totally transformative only in the Horn of Africa but remained very confined to the coastal areas for quite some time. Scholars have generally noted these different impacts and have described them in differing degrees of detail.

Nimtz, in this important study, has given an interesting and sophisticated explanation and analysis both of the transformative impact of Islam and its political role. For Nimtz, unlike historians and Africanists, is not only concerned with description but more so with explanation of the political role of Islam generally in East Africa but more specifically in a particular locale within Tanzania. And though he is interested in the political role of Islam, he is more concerned with a specific form of Islam, namely the Brotherhood, or the Sufi order. To explain the political role of the brotherhood, Nimtz makes three important assumptions: (1) that the best way of understanding that political role is to analyze the social cleavages of society in which a brotherhood functions; (2) that these social cleavages are reflected in the religious sphere; and (3) that the social and economic interests of the individuals of the group accentuate the social cleavages and thus must be understood to provide a full explanation of the political role of the brotherhood.

Using two conceptual frameworks, that of Dahendorf's conflict groups and social change and that of Salisbury's exchange theory of interest groups, Nimtz examines in detail the development of the Qadiriyya order in Bagamoyo, its social, economic and cultural milieu, to ascertain its political role. To make that intelligible, Nimtz rightly provides adequate background information on the spread of Islam along the East African coast, delineates the nature of religious leadership and authority in East Africa essentially to demonstrate the existence of two types of cleavages: one religious and the other social. The religious cleavage is that between formal Islam and Sufi Islam; the social/cultural cleavage between Arabs who migrated from the Arabian peninsula and settled along the East African coast and that of Africans among whom they lived and with whom they fused. These cleavages had important consequences on the system of stratification: generally Africans tended to gravitate toward the less formal but organized hierarchical Sufi order, whereas Arabs—or Arabized Africans—tended to remain within the framework of formal Islam. Leadership reflected the same type of bifurcation: formal Islam was essentially led by Arab shaykhs and scholars more familiar with literal Islam, whereas Sufi leadership, which did not require the same degree of religious training and "knowledge," tended to be drawn from the African ranks. Thus it becomes quite evident that the social cleavage of society between the Arab immigrants and those assimilated to them and the African population had important effects on the structure of religious leadership, organization and authority. Conflicting interests developed and intensified.

European imperialism, first German and then British, noted these cleavages and conflicting interests and used them to their advantage. While in general imperial leaders were apprehensive of the impact of Islam and its revolutionary potential, they were not averse to manipulating Muslims and accentuating the cleavages. Thus the British found it useful to use the services of Arab shaykhs and scholars and rewarded them; simultaneously, it opposed the Sufi brotherhood. In effect the British in Tanganika and Zanzibar showed a distinct partiality to the Arab ruling elite and thereby intensified the conflict in each of these societies. A further intensification of the religious and social cleavages occurred as a result of British policies: the introduction of European personnel and, later on, Asian immigrants from the Indian subcontinent contributed to the further stratification of African society: in the new stratification scheme Africans remained at the bottom of the social/economic hierarchy.

When political nationalism developed, it was natural for Africans to participate effectively in oppositional politics; within that context, the Sufi order, more organized, thoroughly indigenous and effectively led, constituted an important element in the nationalist movement and contributed to the achievement of political independence.

Nimtz's analysis does not go beyond the nationalist struggle and therefore does not address itself to the possible impact of the Sufi brotherhood on the politics of independent East Africa. But a good reading of this important study, its conclusions and hypotheses (which are worth testing), make it possible to critically analyze subsequent African policies toward Arabs and Asians in Zanzibar and other areas of East Africa as well as the interaction of Islam and politics in Africa generally.

IBRAHIM ABU-LUGHOD

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Insurgency in the Modern World. Edited by Bard E. O'Neill, William R. Heaton, and Donald J. Alberts. (Boulder, Colo.: Westview Press, 1980. Pp. xii + 291. $26.50.)

This volume is composed of an introductory chapter outlining a common research framework, followed by seven case studies employing the framework, which are in turn followed by a concluding chapter drawing generalizations from the case studies. The analytical framework, authored by Bard O'Neill, sees the political system in Eastonian terms of making binding decisions for a society. In this context, insurgency is defined "as a struggle between a nonruling group and the ruling authorities in which the former consciously employs political resources (organizational skills, propaganda, and/or demonstrations) and instruments of violence to establish legitimacy for some aspect of the political system it considers illegitimate" (p. 1). I have to admit that I puzzled over this definition, as organizational skills, propaganda, demonstrations, etc., are to my way of thinking the major tools of any political contest, certainly not in any way primarily associated with insurgencies. Also the implication that the major goal of the insurgent is to "establish legitimacy for some aspect of the political system it considers illegitimate," seems to me a rather curious way of characterizing an insurgency, which to my mind consists of using techniques of paramilitary violence to gain political power. O'Neill goes on to categorize insurgencies into six types: secessionist, revolutionary, restorational, reactionary, conservative, and reformist. Although I can appreciate the subtle differences in these types pointed out by O'Neill, I believe that a fourfold classification—secessionist, revolutionary, reformist, and reactionary—would better capture the major thrusts of insurgent movements.

The most useful feature of the analytical framework, and that part around which the case studies are actually structured, are the key variables determining the success or failure of an insurgency. Basing his conclusions on an extensive review of the insurgency literature, O'Neill suggests six variables which he sees as crucial to the outcome of an insurgency: popular support—how much is there and how is it achieved; organization—scope and complexity; cohesion of the insurgent movement; external support—moral, political, material, and sanctuary; the environment—terrain, climate, communications, religions, ethnicity, culture, size of the country, and quantity and distribution of the people; and the response of the challenged government—the government's counterinsurgency strategy. To these are added a discussion of insurgent strategies: Leninist—a conspiratorial strategy; Maoist—the classical guerrilla strategy; Cuban—a variant of the Maoist strategy (incorrectly described, in my opinion), and the urban strategy—terrorism and guerrilla warfare in the cities.

Seven case studies of insurrections comprise the bulk of the book. The cases examined are: Northern Ireland (1968-1978), by Don Mansfield; Thailand (1962-1978), by William R. Heaton and Richard MacLeod; Guatemala (1954-1978), by Vincente Collazo-Davila; the Tupamaros in Uruguay (1962-1972), by James A. Milller, the Kurdish Rebellion in Iraq (1960-1975), by Paul Viotti; the revolutionary war in Oman (1969-1978) by Bard E. O'Neill; and the insurgency mounted by UNITA against the MPLA and their Cuban supporters (1975-1978), by Donald J. Alberts. Overall, these case studies appear carefully researched in the secondary literature, and do indeed follow a common format to an extent unusual in edited volumes of this kind. I was most impressed by the Miller study of the Tupamaros, which seemed richest in detail and which benefited from additional theoretical material. Also of considerable interest was Albert's description of the ironies involved in Cuban counterinsurgency experience in Angola. There are of course the occasional lapses, for example the characterization of the events in Guatemala in 1954 as "U.S. support of the Castillo Armas coup" (p. 109). In that all or most of the authors have connections with the military, it is not surprising that the case studies steer toward military analysis over political analysis. This tends to give credit to counterinsurgency operations for a large measure of success which governments have had over insurgent movements and to view failure in terms of inadequate responses. Basic analysis of political conditions, while not in any sense ignored, are not given particular prominence.

The conclusion, unfortunately, is rather disappointing. Each major variable related to success is examined and highlights from appropriate case studies are mentioned, but there is little in the way of penetrating analysis of the whys of importance or unimportance of particular variables. Nor is there a systematic discussion of the linkages between the variables, especially as to how the variables may alter in importance depending on the type of insurgency and insurgent strategy. On balance, however, I found the book informative and rewarding reading. Major variables affecting the success and failure of insurgent movements are intelligently discussed in the theoretical framework and applied with skill and discipline by the various authors in the case studies. A stronger conclusion, however, would have been appreciated.

W. C. SODERLUND

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Crisi dei partiti e governabilità. By Gianfranco Pasquino. (Bologna, Italy: il Mulino, 1980. Pp. 181. Lire 5,000.)

In the last few years, the problems of the political parties, the governability of the country, and constitutional reforms have become widely discussed topics in Italy. This book is a timely and often stimulating contribution to this debate. Gianfranco Pasquino reviews and analyzes American, Italian, and other Western European literature on the evolution and crises of political parties and party systems as well as on the governability of advanced industrialized democracies. A major value of his book is its focus on the Italian political parties and political system in a comparative context. It is composed primarily of four related papers presented by the author in 1980, which results in some repetitiveness. While this is a little bothersome, it is not a serious defect.

Pasquino emphasizes the particularly central role of Italian political parties in structuring and supporting democracy as well as the special nature of the Italian party system: namely, that a confessional party has been the major party of the center as well as the main governing party for 35 years, and that the strongest opposition party is a communist party.

Pasquino focuses on three major problems of Italian parties: (1) the crisis of representation in terms of their decreased ability to mobilize political participation, their inability to respond adequately to the new demands which are represented by collective movements, and a lower level of identification with parties; (2) decision-making paralysis; and (3) the lack of internal party democracy in the PCI (democratic centralism) as well as the DC and PSI (oligarchic-bureaucratic rule). The problems of the political parties are in Pasquino's view central to the crisis of governability in Italy; he emphasizes the weakness of the executive and Parliament (both subordinated to the parties) and the inefficiency of a bloated bureaucracy which has been colonized by the parties.

Pasquino argues that the Italian political system should be liberated from parties in two ways: (1) parties should have less control over the government; and (2) there should be greater turnover of personnel in these positions. In other words, the hegemony of the parties over public policy and their colonization of public institutions must be lessened.

Pasquino frequently returns to the special nature of the Italian party system and the concomitant lack of alternation of power as the core problem for the governability of Italy. He argues that the lack of alternation leads both to (1) ineffective governing and decisions not being made since the governing coalition is not under a threat of losing power; and (2) an irresponsible opposition which, since it cannot hope to become the governing force, puts forth unrealistic proposals pleasing to its electorate, but which have no chance of being enacted.

Pasquino believes that an alternation of power and competitiveness between coalitions led by the Christian Democrats and coalitions led by the Communists is of utmost importance. Pasquino views the PCI as evolving in a "social democratic-national" direction and the DC as evolving in a "lay-conservative" direction. The great emphasis that Pasquino places on alternation would have made useful a more detailed examination of the evolution of the PCI and DC as well as the likelihood of alternation. Is this really a possibility in the near future? What are the national and international constraints preventing it?

Though Pasquino cautions against hasty action on constitutional reforms and against overreliance on them, he thinks that constitutional engineering in Italy may eventually become necessary. In connection with his emphasis on alternation, he discusses the possibility of changing the electoral system (which might lead to the formation of electoral coalitions) and of Italy's becoming some kind of presidential republic (which might result in competition between opposed coalitions in any direct election for president).

In sum, Pasquino believes there is a crisis of governability in Italy; in his view, the Italian political system has a serious, but curable, illness. This book attempts to provide some answers but also to raise questions for further discussion and research. Though one may disagree with some of Pasquino's proposals, his book is a valuable aid for thinking about the future of the Italian political system.

DOUGLAS A. WERTMAN

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The French National Assembly Elections of 1978. Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1980. Pp. 243. $7.25, paper.)

The French National Assembly Elections of 1978 is a volume in a series which will study a minimum of two national elections in 19 nations. This is the second work in the "At the Polls" series of AEI to deal with national elections in France. In 1975, a volume analyzing the presidential election of 1974 was issued. The two volumes match up well in their coverage of the major aspects of the electoral processes for the selection of the executive and legislative branches of government. For example, both cover the reasons for the near-successes of the political left at the polls in 1974 and 1978, and both include extensive information about the roles of the media in French political campaigns.

By including such common topics, the two works provide a useful base of data for developing a sense of direction about French politics. While the more recent volume provides coverage comparable to that in the previous work, it also includes a chapter on the role of women in French politics. This addition will become a regular part of all future "At the Polls" studies.

All publications of AEI are directed to various publics. In addition to professional social scientists with an interest in contemporary politics, AEI publications are addressed to that portion of the general public interested in public affairs and wanting more depth of analysis than is provided by the popular media. *The French National Assembly Elections of 1978* is thus useful to a variety of client groups.

The volume presents much interesting detail about French constitutional requirements and statutory provisions and their impact on political behavior. At the same time, the contributors usually take care to explain such material in a way that a more casual but fairly well informed reader is not troubled by too much professional jargon or detail. For example, the charts in the appendix which deal with election results are organized in a way that readers with various levels of sophistication may use them meaningfully.

While the writing and editing shows a sensitivity to the needs of various political audiences, there are a few lapses. From time to time some unexplained terms are used which may be puzzling to many readers. For example, Michel Rocard's socialism is referred to as contrasting with that of François Mitterand, but the differences or similarities are not explained even briefly. A major portion of the book's audience no doubt knows who Mitterand is and what his positions on policy issues are, but do as many know who Rocard is and how he differs from Mitterand? A few sentences could give additional clarity to a valuable work.

The most valuable analysis in the book comes from the perception of French politics as evolving into a system which is dominated by four political groups. The media in the United States emphasizes a simple left-right split. There is also frequent speculation about the consequences of the left gaining political power. While the editors did not foresee a leftist group gaining power in the near future, that has happened. Mitterand's victories do not invalidate the concept that four parties are the key to political power in France, and there is still no evidence that either the left or the right are really unified political organizations.

AEI "At the Polls" studies in the past have proved to be thorough, accurate and useful to a variety of audiences. *The French National Assembly Elections of 1978* continues this tradition. The coverage is objective and provides a most useful check on media coverage of recent public events and politics in a nation of importance to a sizeable segment of the American population. This most recent volume continues to provide evidence of the value of the "At the Polls" series.

C. F. POKE

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Rebels and Revolutionaries in North China, 1845-1945.
By Elizabeth J. Perry. (Stanford, Calif.: Stanford University Press, 1980. Pp. xiii + 324. $25.00.)

Elizabeth Perry began her doctoral dissertation research intending to discover the origins of the Chinese revolution. More specifically, she was seeking the roots of that revolution in traditional Chinese peasant rebellions. Her approach was to examine the recent history of a local area—Huai-pei, a geographical region between the Huai and Yellow rivers in North China—which had a well-established tradition of peasant insurrection and which also served as a communist base prior to 1949. By conducting case studies of the Nien and Red Spear rebellions and also the communist movement in the area, she hoped to confirm the continuity between traditional rebellion and modern revolution that many, without conducting the painstaking research required, had previously postulated. The results of her research are now available in this impressive book.

Those who eagerly peruse this book in search of support for standard interpretations of the relationship between rebellion and revolution current in Asian studies are likely to emerge as depressed as Perry herself became upon discovering, early in her research, that a local area's tradition of rebellion could actually serve as a hindrance rather than an aid to revolution. The data she unearthed suggested the necessity of employing a different, rather more complex approach to rebellion and revolution than had been her original intention.

Her revised approach is best characterized as ecological and structural. Perry sees the persistent recurrence of peasant violence as a means of rational adaptation to a harsh environment. In Huai-pei, two basic organized and interdependent survival strategies developed. The *predatory strategy* entailed illegally expanding the resources of some members of the community at the expense of others. The *protective strategy* was an effort to preserve one's belongings in the face of predatory threat. But these adaptive strategies only took shape in the context of the social structure of the local area. To understand the particular nature of the peasant response to economic uncertainty, one must see peasants less as individuals and more as members of socioeconomic groups, with class distinctions of most importance, and clan and settlement divisions also playing crucial roles in the organization of conflict.

Perry's ecological approach, with its concentration on one local area over a 100-year period, is most persuasive when she is questioning some generally accepted interpretations regarding the relationship between peasant unrest and social structure. By looking at peasant violence from the standpoint of the participants themselves, she finds that rebellion was largely an extension of ongoing modes of local competition for scarce resources. In this dynamic, the secret society, as her chapters on the Nien and the Red Spears show so well, should not necessarily be seen as the organized opposition to Confucian orthodoxy or the representative of the poor and oppressed, both common themes in previous literature on the subject. Still less should the secret society be viewed as the key to a general theory of Chinese peasant rebellion. While secret societies did serve at times to advance the interests of the less favored classes, at other times they might play a negligible role in peasant rebellions or be "associated with a conservative, defensive posture on the part of relatively affluent individuals and communities" (p. 150).

The relationship between the region's history of rural insurrection and the success of the communists is also a complex one. Because the predatory and protective survival strategies were firmly rooted in local social structure, the communists found that support from local collectivities was forthcoming only when communist strategy paralleled local interests. For example, an emphasis on land reform and the mobilization of men, while perceived as a threat by protective groups, attracted predators eager for a stable income and the possibility of social change. On the other hand, an emphasis on establishing a stable base area against the Japanese and Kuomintang found communist strategies more closely in tune with those of protectors than of predators. But the victory of the Communist Revolution could not be obtained by relying on the precarious support of either predators or protectors. The success of the revolution depended on meeting the socioeconomic needs of the peasantry so that traditional survival strategies would no longer be necessary. And, in fact, as the revolution progressed, the continued existence of predators and protectors, with their parochial outlooks, became an obstacle that had to be overcome before local society could be restructured. Thus, Perry suggests, at least in Huai-pei there was a clear discontinuity between rebellion and revolution.

The merits of an ecological-structural approach are clearly brought out in Perry's book. A researcher focusing on peasants as participants in organized survival strategies dating back to the precapitalist period is better able to explain traditional rebellion than are the moral economists, who emphasize the security of the traditional "corporate village." The advent of capitalism can then be seen as one more crisis—albeit a much more serious one—for locally entrenched rural institutions to overcome. Moreover, the approach sensitizes us to the tremendous variation that characterizes peasant society. As Perry says, addressing the relationship between rebellion and revolution near the close of her study, "There was no single legacy of traditional Chinese rebellion, but many such legacies, each adapted to the particular ecological exigencies under which it evolved and each differing perhaps in its suitability to the service of modern revolution" (p. 257).

This emphasis on complexity and variation is ultimately both the strength and the weakness of the book. Since the ecological perspective is used to account for enduring traditions of peasant violence in certain geographical areas, we will need many more case studies, of regions other than Huai-pei, to begin to make generalizations about the relationship between traditional rebellion and modern revolution, even if we were to limit ourselves to the Chinese context. In the meantime, Perry's well-written and thoroughly researched book has raised some very interesting questions and challenged some widely held theories.

STANLEY ROSEN

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Science Policy: USA/USSR: Vol. 1, **Science Policy in the United States.** By Nat C. Robertson. (Washington, D.C.: National Science Foundation, 1980. Pp. x + 201. $5.00, paper.)

Science Policy: USA/USSR: Vol. 2, **Science Policy in the Soviet Union.** By Paul M. Cocks. (Washington, D.C.: National Science Foundation, 1980. Pp. xii + 331. $6.50, paper.)

These two monographs dealing extensively with science policy in the United States and in the Soviet Union result from the activities of a Working Group on R&D Planning and Development established by the U.S./USSR Joint Commission on Scientific and Technical Cooperation. The Joint Commission and the Working Group have been active since 1973 and these monographs are the product of the United States' participation. Specifically, the monographs are based on information developed from exchange visits, reports, specific questions and answers within the U.S./USSR Working Group, and other external sources. The data and information were compiled originally by Battelle Institute's Columbus

Laboratories and then drafted in text and monograph form by the authors, Nat Robertson (United States) and Paul M. Cocks (Soviet Union). Though funded by and prepared for the National Science Foundation, the monographs *do not* reflect an official American position and *do not* represent a joint product of the working group. As such they represent an interim stage and an effort from the U.S. side in an ongoing joint process growing out of the May 1972 Nixon/Brezhnev Agreement on Cooperation in Fields of Science and Technology.

Separately and combined, these treatments of science policy in the United States and the Soviet Union *provide a needed and rather comprehensive overview of the environments, organizational structures, decision making and evaluation processes, research institutions, and use of basic and applied research* in the two societies. While Robertson's monograph of the United States and Cocks' monograph on the Soviet Union are not presented and organized in parallel fashion (i.e., a shared, detailed conceptual framework or set of analytical categories), they do focus generally on common topics. Each monograph deals with (1) the political and economic "environments" of science policy and research; (2) the basic national, state, and/or regional governmental organization vis-à-vis scientific research; (3) processes and strategies for resource allocation decisions and evaluations on both applied and basic research; (4) the roles and nature of industrial, university, and nonprofit research institutions and their relationships with government; (5) science planning processes and mechanisms; (6) patterns of technological diffusion and utilization; (7) incentives and obstacles for innovation; and (8) current issues surrounding scientific research and development in each political/economic system. Volume 2 concludes with a 33-page general discussion of the *similarities and differences* between the U.S. and USSR in these eight topical areas.

While both monographs are broad in coverage and reasonably detailed, there are *three* areas which might have had more attention and/or further development:

1. Even though the two monographs are organized around the same general topics, comparison of the two systems (U.S. and USSR) would have been facilitated by a more detailed and analytical conceptual framework (e.g., specific environmental, process, output, and impact variables related in a common model adapted to both countries). The concluding chapter in volume 2 (pp. 298-331) deals with similarities and differences between the systems, but here even a chart juxtaposing the U.S. and USSR would have provided clarity and made comparative analysis easier.

2. The chapters in each volume dealing with current issues could have been made more comparable, including the expansion of the United States' issues discussion to match the breadth and detail of the discussion of current issues in the Soviet Union.

3. Substantively, although brief attention is given to the impacts of political culture, economic systems, and ideology on scientific research and development in the U.S. and the Soviet Union, the monographs would have benefited from perspectives and knowledge already developed in the *sociology of science.* Models and existing research from that field as well as research and conceptual frameworks from political scientists' *studies of the determinants of policy-making processes and policy outputs* would have added greater precision and insight to the analysis.

Despite these problems, the two volumes in *Science Policy: USA/USSR* constitute a valuable resource for political scientists interested in science and research policy and in cross-national comparisons of public policy. Moreover, they are clearly written, highly readable, and provide a thorough summary and compilation of information, issues and research on the subject matter. As such, either or both volumes would be an excellent introduction for scholars to the issues and processes of science policy making. As texts in a course or graduate seminar on science and technology policy, they would be ideal and effective. Combined, they provide a base for comparative studies, but each volume could stand and be read alone with little loss in value. In sum, *Science Policy: USA/USSR* constitutes an important and useful addition to the still-growing, still-emerging literature of a field of inquiry that bridges the scientific disciplines.

DEAN SCHOOLER

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Eurocommunism: The Ideological and Political-Theoretical Foundations. Edited by George Schwab. (Westport, Conn.: Greenwood Press, 1981. Pp. xxvi + 325. $25.00.)

There is little justification for collecting in an expensive volume these papers presented at the third CUNY Conference on History and Politics, held in October 1978. The purpose of a professional conference normally is served by presentation of papers that later can be made available to interested parties; not all, even of good quality, need to be preserved for posterity in a bound volume.

Some attempt was made to give the papers a coherent structure by grouping them into two sections, the first on "the political context" and the longer, second section on "the ideological and political-theoretical foundations." They may all, however, have been less pretentiously arranged as country chapters. The first section, consisting of three articles, provides no political context at all. Instead, there is an opening statement in which Rudi Supek not surprisingly counterposes the creative Marxism of the Yugoslav *praxis* group to the positivism of contemporary Soviet Marxism-Leninism. R. V. Burks chooses to emphasize the power factors that have influenced European communist revisionism, using the Italian communists as his leading example. The third article, drafted by Peter C. Ludz who died shortly before the conference and to whom the volume is dedicated, deals with the influence of Eurocommunism on the Social Democratic party in West Germany.

Part 2, on the ideological and political-theoretical foundations, contains two articles on French communism by Bernard E. Brown and Annie Kriegel; two on Spanish communism by Eusebio M. Mujal-Léon, offering an historical approach, and Juan J. Linz, an electoral one; a nice rundown of Portuguese communism by Kenneth Maxwell; and finally Franco Ferrarotti's article on Italian communism. In his opening remarks, Ferrarotti captures a quality of Eurocommunism that has intrigued me, and may account for both the overproduction of works on the subject and the elusiveness of generalization on its theoretical content and political potential:

> There is something challenging as well as irritating about a discussion of Eurocommunism mainly because one feels that Eurocommunism is real and at the same time elusive. Eurocommunism is certainly a reality—its political, economic, and theoretical implications seem bound to become more and more important—but a clear-cut conceptualization of such a reality, at least for the time being, appears to be difficult if not impossible (p. 157).

What specifically, then, is the reality and theoretical content of Eurocommunism considered as a general phenomenon?

Brown addresses this question in his afterword in which he identifies three major respects in which the European CPs have changed significantly. First, they have called into question the Soviet model and in some cases have flatly rejected it. Second, they have dissociated themselves from specific Soviet foreign policy decisions. And third, they have shown an attachment to parliamentary democracy, freedom of criticism, and pluralism of political parties. Brown points up the extent of the change by contrasting contemporary Eurocommunist positions with the post-World War II theory of "popular democracy" in east Europe. At the time, the communist parties also asserted that socialism could be achieved by democratic means without having to go through the transitional stage of the "dictatorship of the proletariat," but they significantly did not criticize the Soviet model or Soviet foreign policy. There is a difference, then, Brown writes, between the present and the past positions of the European communist parties even if there are doubts about the authenticity of the new terminology. Brown's reference to the theory of "popular democracy" is pertinent, but he would have been better served by reference to the communist party programs that proliferated in the early 1950s in *democratic* countries outlining peaceful and parliamentary roads to socialism. In 1952, for example, the Communist party of Great Britain adopted a program called "The British Road to Socialism," revised in 1958 after the twentieth Soviet party congress, in which it was stated that "using our traditional institutions and rights, we can transform Parliament into the effective instrument of the people's will, through which the major legislative measures of the change to socialism will be carried." I do not recall that Western scholars were excited at that time by the revisionism in Western communism. They probably attached little credibility to the programmatic statements of the communist parties of the Stalinist or immediate post-Stalinist periods. Why the greater measure of credibility (sincerity?) accorded today's Eurocommunists? Is it possible that the leopard has changed its spots, or is it the same old leopard in drag?

It's the latter, apparently, according to Brown who concludes that, whatever the ideological ferment in the parties, the " 'foundations' are intact," viz., the notion of class struggle and the hegemony of the working class led by a vanguard party. It is an ideology, he continues, that boils down to seizure of power without any positive indication of the kind of society that would follow. But if the foundation were not intact, what, one might ask, would distinguish the communist from the socialist parties between which, Brown writes, the distance is as "undiminished" and unbridgeable as ever?

To ask the question another way: in what sense can Eurocommunism lay claim to its Marxist revolutionary heritage without capitulating to the reformism experienced by the Marxist socialist parties in the late nineteenth and twentieth centuries? Only, it would seem, by maintaining the essential values of the Marxian tradition in the present historical period. Marxist-Leninist ideology, which serves as a touchstone for commentators on the measure of Eurocommunist deviance, has served precisely the opposite function attributed to it by these commentators. The ideology has been manipulated by the Moscow center, from which the communist parties have historically derived their legitimacy, to inhibit rather than facilitate revolutionary politics. Having distanced themselves, however uneasily, from Moscow, the parties are freer to pursue a policy of radical change within the particular constitutional structure of their respective societies. In so doing, they are compelled to devise a set of principles to attract a wider constituency, but principles that would conserve core values of Marxism and set them apart from reformist parties. The analogy with Yugoslavia—or China—comes readily to mind, but the crucial difference is that the Eurocommunists must do so as oppositional, not ruling parties. Both the theoretical and political obstacles confronting the communist parties are formidable, not the least of which is the taint of Stalinism that never may be erased.

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The Italian Communist Party: Yesterday, Today, and Tomorrow. Contributions in Political Science, No. 46. Edited by Simon Serfaty and Lawrence Gray. (Westport, Conn.: Greenwood Press, 1980. Pp. xii + 376. $29.95.)

"While politics are complex, Communist politics are twice as complex as ordinary politics, and Italian politics are many times more complex than those of any other country." This statement occurs in the concluding chapter of this series of essays on the Italian Communist party (PCI) which document the high degree of sophistication, conceptual and tactical agility, and contradictions of Europe's most successful nonruling communist party.

In exploring the PCI's domestic strategies from World War I—the period of Antonio Gramsci, one of the party's main ideological fathers—and the party's "foreign policies" following World War II, this book looks at the fundamental dilemma (discussed in Blackmer's and Tarrow's seminal *Communism in Italy and France*) which has ubiquitously faced communist and socialist parties in Western Europe: to what degree can they remain "revolutionary," in at least the minimal sense of seeking to transform their societies, and not be coopted by or integrated into the societies and institutions and values that they seek to change?

The perspectives of the eleven authors from four different countries and several disciplines vary on details, but there is a broad measure of agreement, which in part may be explained by their all having been associated—past or present—with the Johns Hopkins Center of Advanced International Studies in Bologna.

By the time the PCI had been founded by Gramsci and Palmiro Togliatti in 1921, Gramsci had already pinpointed a recurring theme: the party wanted to remain true to Leninism while at the same time trying to be a national and popular movement with appeal to all segments of Italy. Gramsci tried to reconcile the two, but Leninism scares away large social groups while electoral tactics dilute the revolutionary substance that makes the party unique.

The book traces the roots of the pragmatic gradualist path that has characterized the PCI since Togliatti returned from the Soviet Union in 1944 to bring the PCI into a national coalition government to fight the Germans. This strategic move away from a Leninist-cadre party in the direction of a modern mass party, did not set well with the older militants, but prepared the way for the *Compromesso Storico* (compromise with the Christian Democrat government) 30 years later, and the PCI's winning 34.4 percent of the vote in 1976 and "unofficially" becoming part of the DC-led government until the beginning of 1979. In one of the book's more informative chapters, Gianfranco Pasquino concludes that the "historic compromise" has deprived Italy of some of its "best potential elites" because their location in the Communist orbit precludes their full participation in the government. At the same time, real power remains firmly in an informal coalition of DC and PCI elements. Since the DC and PCI—along with the Socialists—now represent more than 80 percent of Italy's total vote, and since there seems to be little possibility that the DC will be dislodged or the PCI will gain power, the result is a hardened status quo.

Two chapters depict the evolution of U.S. policy toward Italian communism from the Second World War period, which culminated in supporting the social forces which more or less had backed fascism. Ennio De Nolfo writes: "The Americans failed to use the exceptional opportunities created by the Italian defeat to provide an alternative not only to fascism but also to communism." After expansion of the cold war, the American policy of anticommunist containment permitted no ambiguities, thus precluding a genuinely reformist alternative and hardening Italian politics into its two postwar blocs.

In discussing the PCI's role in international relations, the volume traces the transition from loyalty to the Soviet Union to changes induced by de-Stalinization, events in Poland and Hungary in 1956, and formulation of the "Italian Road to Socialism" in 1964 characterized by relative independence from the Soviet Communist party. While the Soviet invasion of Czechoslovakia and other events culminated in PCI Secretary Enrico

Berlinguer's call in 1973 for collaboration with all actors in the international system, PCI's fundamental dilemma continued to surface: how to maintain solidarity in the international communist movement while playing the pacesetter role in the "Eurocommunism" thrust which emphasized autonomy from Soviet dominance, reaffirmation of the parliamentary road to government power, and acceptance of social/political pluralism. Eventually the PCI approved Italian participation in NATO and the Common Market; closer relations with the most independent-minded communist parties of eastern Europe and such Russian dissidents as Medvedev; a surprisingly moderate policy toward profitable private enterprise in Italy and multinational corporations; and a policy on world economic development which recognizes the need for "austerity" and a "different kind of growth" in a time of impending scarcity.

In the final chapter Pierre Hassner reiterates a number of characteristics of the PCI that have made it one of the world's most intellectually creative as well as electorally successful communist parties: its penchant for avoiding confrontation both domestically and internationally, seeking instead a "third way" where there is maximum convergence; its delicate interrelationship between domestic and foreign policies, breaking new ground especially when international tensions are low; its give-and-take between militant rank-and-file members and more pragmatic leaders; its maintenance of global links with "revolutionary forces" and "liberation movements" to balance off domestic politics of accommodation and possible labeling as "social democrats."

As is usual in collections of essays by different authors, parts of this book are repetitive, but it is one of the most succinct and authoritative current works on the Italian Communist party available in English, and includes a comprehensive bibliographic essay.

BELDEN H. PAULSON

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Peasant China in Transition: The Dynamics of Development Toward Socialism, 1949-1956. By Vivienne Shue. (Berkeley: University of California Press, 1980. Pp. xvi + 394. $29.75.)

In *Peasant China in Transition* Vivienne Shue attempts to explicate the nature of the relationship between the Chinese Communist party (CCP) and the peasantry during the early phase of post-Liberation state development. Her central question is how the new People's Republic of China led a peasantry steeped in parochial attitudes and untutored in Marxism-Leninism toward socialism. Shue offers a number of fascinating answers by focusing on the CCP's mobilization of the peasantry in the South China provinces of Hunan and Hubei in the years 1949-1956.

Chapter 1 takes a suggestive look at how the CCP and its revolutionary army reestablished order in the chaotic countryside in 1949-1950. According to Shue, the CCP did this by stopping the plunder of defeated Guomindang Army troops and by offering relief grain and relief work to the dearth-ridden villages. Taxes and rents were reduced in less than radical fashion, and these moderate means of leveling were carried out in ways that benefited the poorest peasants without driving the middle and rich peasants into the camp of a landlord-based counterrevolution. At the same time, the CCP cadres tried to enlighten the rural people in ways that developed a primitive sense of class consciousness.

The CCP's rural taxation methods are explored in a third chapter. Taxation was less than central to the state's efforts to bring about revolution. Shue shows that tax policy even served to stimulate individual peasant household economic orientations which were not necessarily in line with the currents of state collectivization. This chapter does a nice job of outlining how peasants avoided new taxation demands.

In the fourth chapter Shue explains how the CCP drew the peasantry into mutual aid teams to increase production and improve purchasing power. These teams respected the private household economy, and yet somehow constituted the stepping-stone to rural socialism. Chapters five and six make up the heart of the book. They deal with rural development, paying special attention to rural trade and finance. They are filled with rich evidence and deep insights into peasant society and political economy in South Central China.

On the one hand, Shue is concerned with how the PRC devised programs to restrain what she believes CCP officials perceived to be "petty capitalist trends" in the countryside, so as to prevent the redevelopment of socioeconomic gaps between rich and poor peasants. On the other hand, Shue explains how the PRC accomplished this policy goal while simultaneously cultivating incentives for peasant cooperation.

Chapter five, on rural trade, underscores the difficulties faced by state marketing coops and trading companies in their attempt to monitor and mold peasant consumerism and to restrain the peasant penchant for free exchange of goods in profit-oriented markets. Chapter six, on rural finance, surveys the pitfalls of CCP attempts to create credit coops in the villages, and here too the emphasis is on the extent to which the financial institutions of the PRC state could regain some control over "free capital" in the rural areas in the face of real peasant reservations about the perceived benefits of such participation. In the end, Shue concludes that through restructuring peasant choices the new PRC state *was* able to develop a rural economy in which peasants went along with two state goals. The first goal was that state leadership in supply and market coops should replace independent peasant entrepreneurship as the key determinant of market prices (primarily grain), and that state leadership could win peasant support for this price-setting policy by offering adequate incentives of a real materialistic nature. The second goal was that private profit making, while tolerated, was to be conducted within a societal-state context that redirected its *outcome* to fall in line with public, or collective, needs. Most interestingly, Shue concludes that the majority of peasants felt comfortable with this development—which at bottom was a blow to the rich peasant economy.

The seventh chapter focuses on the formation of the Agricultural Production Cooperatives to resolve the problems of low agricultural productivity. Here Shue details the determination of the Maoist center to move rapidly ahead in agricultural collectivization. The central thesis seems to be that under Mao the CCP was able to carry out collectivization which simultaneously leveled local inequalities and left more revenue in the hands of those political leaders looking to build up the industrial sector.

The conclusion offers a number of original ideas about the CCP as a force of agricultural modernization, as a manager of class conflict, and as a benevolent central government. The overall thrust of Shue's book is that despite the difficulties created for the PRC by the residual effects of earlier imperialist-induced social impoverishment and the post-Liberation intransigence of the small peasantry, the Chinese Communist party was able to move China along the road to socialism without seriously undermining the legitimacy its rulers had achieved in the pre-1949 revolution.

Peasant China in Transition is an important contribution to understanding China's experiment in revolution. The conclusion offers two attractive insights. One challenges the Skinner-Winckler model of CCP influence at the mass level. Shue wisely takes issue with the notion that the CCP relies on ideology to get peasants to act out its interests while employing material incentives only when ideological appeals fail. She correctly contends that the CCP has consistently placed greater stress on concrete appeals than coercion in its efforts to mobilize the peasantry. Attractive too is the notion that the early PRC government seemed to be aware of the importance of not placing too much pressure on preexisting local social networks and personally based forms of rule in its attempt to gain popular compliance with its own extractive goals. Clearly—and here I agree completely—the early Chinese Communist state harked back to the days of the good dynasty when penetration and extraction were tempered by benevolence and compassion. There is no parasitic state here, at least not for the first years of Communist rule.

In some ways *Peasant China in Transition* does for the field of China studies what Marcuse's *Soviet Marxism* did for the Soviet field, though its overall thrust is less critical and more subtle. Shue's focus on the CCP's attempt to regulate the anarchy of the peasant-based free market suggests that the CCP could better realize its own goals when these were made to be congruent with peasant needs. Shue tends to think that under Mao Zedong the CCP in South China furthered the needs of rural petty-capitalists whose self-interest could be fulfilled in a political context that favored them without damaging the poorer village people. Shue's novel and controversial interpretation is that petty-capitalist enterprises were not incompatible with the cooperative social endeavors that could make up the building blocs for a better society, and this interpretation may be correct for the "Childhood of the People's Republic."

Shue also understands what so many Western social scientists who study China fail to understand: that peasant China was not ready for a transition to socialism in 1956, and that the emergent CCP dictatorship had to respect this reality unless it wanted to alienate the rural people and raise passive resistance to its increasingly radical schemes for modernization and state development. Fortunately, Mao stopped short of the tactics of Stalin (but just how far short?), and the CCP did not drag peasants screaming into its own schemes for wealth and power. Shue's historical telescope is short. One can only hope that in a future work it will be extended to the years 1956-76 so that we can receive even more enlightenment about why and how peasants have staved off socialist modernization in People's China.

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Background to Crisis: Policy and Politics in Gierek's Poland. Edited by Maurice D. Simon and Roger E. Kanet. (Boulder, Colo.: Westview Press, 1981. Pp. xviii + 418. $27.50.)

This is a "let's do a book on. . ." kind of book and the editors admit as much, having conceived of the volume "in the coffee shop of Warsaw's Hotel Europejski, "when they met for the first time in the fall of 1976 (p. xii). It has no theoretical framework, no overall organization and, one is tempted to say, no visible purpose apart from the desire to have a book published. The role of the editors, like so many others of this kind, is limited to simply sticking a number of articles, here called chapters, between two hard covers. There are fourteen of these, divided into five parts: (1) introduction, (2) the party, institutions, participations, and the policy process, (3) group activity and attitudes: their impact on policy, (4) issues in economic and foreign policy, and (5) conclusion.

Despite the lack of any editorial effort, the book is not completely without value because of a few of its individual contributors. The introduction, composed of one descriptive and informative chapter by Adam Bromke, discusses Gierek's economic and political problems, the growth of political opposition and its organization and publications, and the disarray in the party. Bromke does not attempt to explain "why" from any analytical point of view and gives us what is basically a good newspaper article. The first chapter in the second part by Sarah Meiklejohn Terry on the *Sejm* (parliament) is of the same kind as Bromke's, pointing out the growing importance of this legislative body, especially in the committees' work and reviewing the debate in the Polish literature as to its functions.

In chapter 3 we finally come to political science. Nelson continues his application of the theory of the dichotomy of the horizontal versus vertical integration to the Polish case. Dichotomy in the complexity of communist politics often prevents reforms by blocking their implementation by the conservative subnational levels. Bielasiak on the Polish political elite (Ch. 4) is the most scientific, although one could question his blind reliance on figures alone—e.g., is the percentage of workers in the Central Committee a decisive index of their impact on the policy formulation? The problem of the elite is its continuous vertical recruitment by which the apparat remains isolated from the general public as well as from the technocrats. So much for Burnham, Ellul and Ludz.

The third part opens with a chapter on Polish students, by one of the editors, Maurice D. Simon. Based exclusively on Polish sociological research, carefully documented and argued, it nevertheless commits the same mistake as most of the Polish sociologists do. It shows the failure of the government to create a socialist "sociocentric" man and shows that most students are rather "egocentric." Then how can one explain the "sociocentric" behavior of students in 1956, 1968 and 1981? It proves the students' willingness to sacrifice against but not for communism. Jane Leftwich Curry (Ch. 6) gives a competent discussion, even if one theoretically weaker than her previous works, on the dilemma of journalists torn between professionalism and the political demands of their work. In chapter 7 Christine Sadowski on the voluntary associations is informative and descriptive, but little more. Could one not at least use Marxist theory to build a framework for the discussion of the importance of these bodies to the socialist state?

Dale R. Herspring again returns the book to a professional level. His analysis (Ch. 8) of the function of the Polish military in the decision-making process, well rooted in theory and excellently documented, rightly points out its limited role in policy formulation, because of its subordination to the Soviet command, and at the same time its significant role in policy implementation. Vincent Chrypinski (Ch. 9) gives a well-written, informed and knowledgable review of the history of church-state relations and, in passing, touches on the most important reason for the constant crisis of communism in Poland—lack of legitimacy. Andrzej Korbonski gets his first name misspelled but opens part 4 with a discussion of Polish agriculture. Like Chrypinski, Korbonski is an old hand in East European studies and knows his subject well, easily proving his point that Polish agriculture reached "the upper limit of its productive capacity, and further increases in output must be accompanied by major structural changes" (p. 289).

John P. Farrell (Ch. 11) and Zbigniew M. Fallenbuchl (Ch. 12) do a very competent, if not difficult job, of showing the economic underpinnings of the present political crisis: the first, concentrating on the structural changes needed, the second, on the Stalinist model of economic development which proved totally unsuitable for Poland. Roger E. Kanet (Ch. 13) starts well in terms of a theoretical framework on Poland in the context of East-West relations, but further on drifts into a pure description of events. He shows too some ignorance of Polish internal affairs; for example, he confuses different opposition groups with one another, i.e., KOR, ROPCO and KSS-KOR. But at least his conclusion is one of the few which predicts the present revolution, called by him "chaos." Simon, who in chapter 14 is supposed to provide a "conclusion," ignores all the contributors to this, his own, volume and instead writes an unrelated evaluation of Poland in the 1980s, drawing on the research of other authors.

This work brings little to the professional and not enough to the general public. It raises many questions, not necessarily about Poland, but rather about why some people are called editors and others are called political scientists. The price of the book is about almost as much as the yearly subscription to the *Christian Science Monitor*, the latter most certainly a better bargain.

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Newspapers and Democracy: International Essays on a Changing Medium. Edited by Anthony Smith. (Cambridge, Mass.: MIT Press, 1980. Pp. xiv + 368. $25.00.)

The study of the mass media has never been integrated very well into the study of larger political processes. This is especially true in the United States. American political science has focused primarily on the impact of the media on particular political attitudes and voting decisions. Sociologists have focused on the internal processes of news organizations. There has been little systematic attempt to address the kind of problem Anthony Smith poses in *Newspapers and Democracy:* to relate changes in the press "to the central public purpose with which the newspaper is associated: the linking of the individual to the political realities of society" (p. x).

"The reader," according to Smith's introduction, "can choose whether to treat this volume as a connected survey of the state of the art in various parts of the world or as a series of separate glimpses at the presses of different countries" (p. xi). I find *Newspapers and Democracy* more a series than a survey. It is a collection of studies on the state of newspapers in various liberal-democratic societies. The articles deal with many facets of the newspaper and its context. Some are concerned primarily with technology and economics, some with the political relations among press, state and party, some with content, some with audience. Almost all are focused on single countries. None deals in a general way with the similarities and differences among the presses of different countries, or with the connections among various contexts and roles of the newspaper.

But given the primitive state of comparative research on the media, it is a major step forward simply to bring together such a wealth and range of material. The lack of general analysis notwithstanding, one comes away from *Newspapers and Democracy* sensitized to the variety of forms the press can take in different historical contexts, the similarities that nevertheless characterize the dynamics of newspaper development, and the range of forces that shape that development. From there it is not hard to begin formulating more general questions about the changing role of the newspaper in liberal democracy.

Consider Giovanni Bechelloni's article, "The Journalist as Political Client in Italy." The Italian press, according to Bechelloni, has an unusual history. Because of a high level of illiteracy, the lack of a large middle class and prosperous working class, and other factors, Italy has never developed a mass press comparable to that of other liberal-democratic societies. The Italian press has been elite-oriented and particularly dependent on the state and the party. This began to change only in the 1960s and 1970s, when economic development made possible the growth of mass circulation. Bechelloni predicts that the Italian press will become increasingly market-oriented, "professionalized," independent of the state and, as traditional party- and class-based channels of political participation weaken, increasingly central to the political process: "One can conclude that a shift in the scene of political debate from direct participation to indirect participation through the media is occurring" (p. 241).

When one considers Bechelloni's article in relation to the rest of the volume, one begins to see how much of the process of change in the press is common to all liberal democracies. The development of the Italian press may be unusual in its timing and circumstances, but the logic of that development and the forces that propel it are familiar. There seems to be, for example—and this is probably the most important general finding that emerges from *Newspapers and Democracy*—a tendency for the press in all liberal democracies to move toward what might be called the "American model," the model of privately owned, market-oriented newspapers, each with a monopoly in its own sector of the market. This tendency has powerful economic roots: it follows from the structure of the advertising market, and is accelerated by competition from broadcasting and by a crisis in production costs. It may also be fueled by a weakening of party alignments, though here only limited evidence is provided. And if it does in fact lead to a "shift in the scene of political debate," this is certainly a development that merits further study. Much of *Newspapers and Democracy* deals with policies designed to restrain this tendency and to maintain, through political intervention, the pluralistic, party-oriented press that is threatened with extinction.

Most of the contributors are associated in one way or another with the newspaper industry, and this is perhaps an important limitation of the volume. Only Curran, Douglas and Whannel's "The Political Economy of the Human Interest Story" is broadly critical of the press. Interestingly, this article is also the only one that deals with the *ideological* context of the press, along with the political and economic.

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British Government and Its Discontents. By Geoffrey Smith and Nelson W. Polsby. (New York: Basic Books, 1981. Pp. xvi + 202. $12.95.)

British Government and Its Discontents is a short political survey of modern Britain in the light of its major problems, basically involving a general weakening in economic productivity, social solidarity, and international influence. This work can be strongly recommended for the general reader because its descriptions are concisely informative and its opinions authoritatively well balanced. As one might expect from a felicitous partnership between an American political scientist and a British political journalist, operating from Berkeley and the *Times,* respectively, the comparisons between the British and the American political systems are sophisticated and insightful.

The capsule analyses of complex subjects like Northern Ireland, membership in the European Common Market, immigration, the civil service, and the operation of Parliament are particularly recommended. Moreover, the study of the British economy is a judicious one, emphasizing the seriousness of its problems, and yet not succumbing to the "gloom and doom" views which predominate in many economic surveys of the country. The incidence of strikes and the role of the trade unions are put in perspective even though union leaders probably should be accorded more credit for their part in checking inflation during part of Labour's period in office.

Political scientists, especially those interested in British affairs, will probably find most useful those sections dealing with the increasing importance and assertiveness of the individual MP, especially with the future operation of the recently organized specialized committees. These are relatively new phenomena and, according to the authors, fit into a general pattern of rendering British government more responsive and more open than it has been in the past.

Since the book was completed in June 1980, one can assume that if the authors had to do it over again, they might have waited a year because the 12 months succeeding its completion have proved to be among the most eventful in the history of modern British party politics. Although Smith and Polsby do note the dissolution of the post-World War II political consensus and delineate the divisions within the major parties, they were not able to observe as fully as they might have the effects of the Thatcher government program and the polarization of the political scene as the Conservatives have moved to the right and Labour to the left. Most important, they were not able to see the emergence of a new party, the Social Democrats, and to weigh its possible impact in transforming the party system. Admittedly, no one could fault the authors for failing to predict that this would be the year the Labour party would split. They might, however, have given attention to the disputes regarding the automatic reselection of MPs and the changed procedure for electing the party leader, which were the immediate causes of the breakaway, since there was already enough prior indication of their potential trouble for the party.

A greater emphasis might also have been placed upon the decline in mutual trust and confidence between parties and classes, and its effect upon the political structure and its operation. This can explain to some degree why complaints about "elective dictatorship" and proposed remedies like the Bill of Rights have secured more prominence today than formerly. In addition, a description of the changed composition of the House of Lords would probably have been illuminating to American readers.

British Government and Its Discontents is, in short, a highly useful work because it does show how the government has adjusted to meet its most pressing problems and to check the resulting discontents. The British malaise is serious, as they point out, but not terminal, and certainly amenable to treatment.

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Political Opposition and Local Politics in Japan. Edited by Kurt Steiner, Ellis S. Krauss, and Scott C. Flanagan. (Princeton, N.J.: Princeton University Press, 1981. Pp. ix + 486. $30.00, cloth; $9.95, paper.)

This hefty symposium volume resulted from a 1976 workshop sponsored by the Joint Committee on Japanese Studies of the Social Science Research Council and the American Council of Learned Societies. Its main purposes were, as one of the editors declares at the outset, to "describe and analyze the spectacular developments in Japanese local politics over the last decade and a half and to contribute to theory building in . . . comparative local politics" (p. 3). Another important, if implicit, purpose was no doubt to fill a gaping hole in the literature on postwar Japanese politics, for this is the second book-length study ever published in English on local politics in postwar Japan. The first was a 1965 opus authored by one of the editors of the present volume (Kurt Steiner, *Local Government in Japan,* Stanford). This is so far the only English-language book that focuses on local opposition politics in contemporary Japan.

Beyond its value as a rare text filling an important gap in the literature, the volume serves its two stated purposes very well. The ten substantive chapters clustered in the three middle sections of the volume span most of the major issues in recent scholarly research and popular debates on the subject: electoral trends, citizens' movements, and progressive local administrations. Moreover, each section opens with an excellent overview chapter authored (coauthored in one case) by one of the editors. Finally, each section contains at least one exceptionally informative and interesting case study: Gary D. Allinson's on variations in opposition politics as seen in two Tokyo suburbs; Jack G. Lewis' on the experience and implications of the pace-setter antipollution citizens' movements in Mishima City; and Krauss' on the politics of progressive pragmatism in Kyoto prefecture under its seven-term leftist governor.

With all its virtues, however, the volume is by no means entirely free from problems common to most works of its kind. Some individual chapters contain generalizations that appear to stand on rather shaky empirical or logical grounds, even if they are not necessarily wrong for that reason. Allinson's statement that purely residential suburbs are more likely to support opposition parties than industrial suburbs (p. 127) or that through their urban work experience young rural migrants eventually become Socialist voters (p. 127) belongs to this category. So does Ronald Aqua's assertion that "decisional and organizational" factors are more important than environmental factors in explaining variations in local spending priorities (p. 371) or that the allocation of central government resources to localities is generally nonpartisan and nonpunitive in its effect (p. 382). (Krauss offers contrary observations in reference to Kyoto; see pp. 397-99).

A more serious flaw of the work is its nearly total neglect of two important topics. One is local politics in rural Japan. While the work tells us a great deal about why opposition succeeded as much as it did in cities, thanks to profound underlying socioeconomic changes, it says little about how or why it failed in the villages, despite the equally if not even more profound socioeconomic changes that have swept rural Japan during the last two decades. This omission is particularly unfortunate because it permits the perpetuation of a simplistic modern-traditional dichotomy, as seen in the characterization of "old politics" versus "new politics" (p. 52) and "patron-client democracy" versus "pluralist democracy" (p. 428 f.). It is thus assumed, probably quite erroneously, in my opinion, that rural voters are by and large "traditional," remain bound up in *kankei* (personal connections) networks, and *therefore* vote conservative, while the opposite is true for urban voters.

The other unfortunate omission concerns the role of prefectural and local assemblies. It is one thing to emphasize the importance of the role played by governors and mayors in the rise of the "new politics"; it is quite another thing to try to explain the strengths and vulnerabilities of the "new politics" or "old politics" without reference to the influence of local assemblies and assembly members. This omission is especially debilitating in Aqua's analysis of national-local variance in spending patterns, as, for example, when he apparently assumes that a mayor's partisan identification independently influences the patterns of revenue acquisition and spending priority setting (p. 375). A mayor's influence in such matters must surely vary not only with his own partisan identification but also with the partisan complexion of the municipal assembly with and through which he has to work to accomplish his policy goals.

Inclusion of a case study or a detailed survey piece on opposition in small-town and village elections in the section on electoral trends—this might have necessitated consolidation of the two overview chapters by Flanagan in the same section—and some discussion of the role of local assemblies in one or more chapters in the section on progressive local administrations would have made this work a better textbook. For a symposium volume, however, it must be said to be a remarkably well-conceived and well-organized collection of exceptionally interesting papers.

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The Limits of Professional Power: National Health Care in the Federal Republic of Germany. By Deborah A. Stone. (Chicago: University of Chicago Press, 1980. Pp. xi + 212. $18.50.)

This is an excellent study of the physicians' involvement in the statutory health care system in West Germany. The major thesis of the book is that West German doctors are far less autonomous than their American colleagues. Among the factors limiting the power of the medical profession that Deborah Stone explores are the fragmentation of medical associations on the basis of specialization and the relationship to the health-insurance system; the competition for fees allocated from a fixed money pool; the control over the provision of medical services by special "surveillance" doctors; and the countervailing powers exercised by miscellaneous associations and institutions involved in bargaining over medical-care provisions. Finally, there is the limitation of power imposed by the public-law legitimation of certain medical groups and the overinstitutionalization of their dealings with the public authorities.

Stone advances two conflicting hypotheses regarding physicians' power: (1) the inherent characteristics of medical practice are so complex and unique that the government's power to make health policy is limited; and (2) the power of the medical profession derives largely from official grants of authority, so that the medical profession —in its pricing and other behavior—is dependent upon the will of the political decision makers. The American situation appears to confirm the first hypothesis, and the West German, the second. The recognition that German medical associations are not omnipotent leads Stone to a critique of the "interest-group approach" to the medical profession; this critique is reflected on the one hand in an excessive repetition of the theme of "corporatization" and, on the other, occasionally setting up straw men: the author charges that scholars taking an interest-group approach generally define medical associations as (purely) private groups independent of the government. But in fact the studies of Eckstein, Naschold and others clearly emphasize the institutionalized relationships and the quasi-governmentalization of parts of the medical profession.

Stone discusses three American concepts about means of achieving accountability: representative democracy, pluralism, and free-market competition, and asserts that none of them has been "appropriate" to control the medical professsion, at least not in the United States. But in Germany, representative democracy figures prominently in social-security elections (which are found ineffectual); pluralism may apply not only in Germany but also in the U.S. because it is not necessarily true that, even in the medical profession, "there is a fundamental incompatibility between the notion of expertise and the notion of competition" (p. 11). What about the custom of getting a second opinion? Moreover, in most Western European countries, pluralism is insured by the coexistence of, and competition between, private and statutory medical-care systems. In the U.S. market competition does not exist; and the only mechanisms controlling the behavior of MDs are informal peer reviews, formal self-regulation, and tort law; in West Germany, these restraints are supplemented by official involvement of consumers, employers, the government, the political parties, *and* the sickness funds in determining and monitoring medical fees and the supply of services.

Stone's attempts at making comparisons, in particular between West Germany and the U.S., are praiseworthy; unfortunately, the discussion tends to weave from one country to another, so that the reader must sometimes strain to know which country is being discussed. Occasionally, too, comparisons are impeded (through no faullt of the author) by the incommensurability of organizational patterns and the unreliability of the available data, e.g., American (i.e., AMA-confected) statistics about doctors' incomes.

It is inevitable that in a complex book such as this small problems and confusions should creep in. Thus there are some errors in translation, e.g., *Bundesmantelvertrag* is not "a federal guideline" (p. 101), but a framework agreement; and a "concerted action" is not "a large group" (p. 153), but rather a policy-making *process* involving groups. Moreover, while it is stated that the "Concerted Action on Health Affairs" involved a variety of groups, including physicians, we are not told how many doctors, nor which medical associations were involved, nor how effective a vehicle this has been for the profession. The "Society for Social Progress" is mentioned without explanation. Similarly, there is a mention of ministerial advisory councils dealing with medical issues, but the nature of group representation, the role of the "independent experts," and the influence of such councils are not discussed. Stone's skepticism with respect to the usual pressure-group approach probably accounts for the fact that there is little discussion of MD alliance-building, of the relationship between medical and other professional (or entrepreneurial) associations, of medical representation on parliamentary committees, or of the development of access to political parties, legislators and civil servants.

Stone asserts that "the assumption of income-maximizing behavior [is] only partially true" (p. 116), but she does not convincingly present other kinds of physicians' behavior motivations. On the other hand, there is the statement (p. 162) that "in Germany, self regulation is grounded in a conception of society as an organic whole, whose constituent parts are large social groups based primarily on economic functions." This statement raises problems because organic groups are not functionally defined, and because the largest "organic group," the Church, was not viewed as economic. More important, since U.S. society is not organic in derivation, does it follow that we cannot have a self-regulatory approach to the professions in this country? Elsewhere (p. 163) the author asserts that the medical profession "derives its collective power primarily from its early willingness to engage in collective action against both the state and private employers . . . and its willingness to organize as trade unions." This, alas, contradicts the author's earlier points about German medical associations being neither separate from the state, nor (because of compulsory membership and statutory tasks) akin to American trade unions.

The last chapter is evocative, for it challenges the reader to derive lessons for America from the German experience. Yet such lessons will be limited, precisely because Germany's experience with its physicians, the place of socio-professional groups in its political system, and finally, the doctors' self-image—all these have derived from corporatist-etatist traditions that have not been replicated in the United States. Still, this solidly researched, well-organized, and intelligently reasoned book about the limits of the power of physicians in West Germany should be of interest to the growing number of Americans who are exercised about the apparent hopelessness of attempts to curb the galloping cost of medical care in this country.

WILLIAM SAFRAN

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Zealots and Rebels: A History of the Communist Party of Czechoslovakia. By Zdenek L. Suda. (Stanford, Calif.: Hoover Institution Press, 1980. Pp. xii + 412. $8.95, paper.)

Part of a series of histories of ruling communist parties, this book traces the developing of the Communist party of Czechoslovakia from its origins in 1921 to the present. Noting that circumstances in Czechoslovakia were quite different from those in which other communist parties came to power, Zdenek Suda focuses on the impact which a democratic political culture has had on the strategies and behavior of communist leaders. Another theme which recurs throughout the book is the problem which Czech and Slovak communist leaders have had in subordinating national considerations to the needs of the international communist movement and the Soviet Union. In the course of a chronological description of the party's actions, the author also provides information on sources of popular support, extent of Communist party membership, and intra-elite conflict. He also discusses the differing importance of nationality issues during the precommunist period, when party leaders used national aspirations to increase support for the party, and the post-World War II period, when Slovak desires for parity within the state and party have proved problematic.

The chief value of the book is as a reference work for persons interested in Czechoslovak history or politics. Although Suda introduces little that is new in terms of interpretation, his book is one of the few works available in the West which focus primarily on the Communist party and the only recent attempt to examine the party's entire history. As such, it supplements the many excellent works on specific aspects of politics or particular historical periods in Czechoslovakia. Although one can argue with certain of the author's judgments, the book provides the reader with the basic facts concerning the main phases of the party's development in Czechoslovakia and an overview of the country's recent history.

It is less successfull, however, as a work of political or historical analysis. Suda occasionally refers to well-known sociological theories, but his treatment of the party's history is chronological and largely descriptive. As a consequence, the book does not explore the numerous interesting theoretical questions which arise from the Czechoslovak experience. Nor does the picture of the party really get beneath the surface. Thus, despite the fact that the book focuses on the party itself, one gets little sense of how the party actually functioned or functions as a political organization in Czechoslovakia. With the exception of the particular case of Slovakia, the author gives very little attention to the activities of regional or local party organizations. Because much of the discussion is confined to the national level, we learn little about how the party's directives and resolutions were carried out in practice among the membership. We learn similarly little about other aspects of intra-party dynamics, beyond the main groupings in the top elite at various periods.

In part, this problem arises from the difficulty of balancing discussion of the party and the broader political context. In certain sections, such as those dealing with the Communist party's actions during the interwar and wartime periods, the discussion could benefit from the inclusion of more information about the activities of other political actors in Czechoslovakia. In others, including those which deal with the Prague Spring and the normalization period, the party itself gets lost in the discussion of broader political trends. In the latter case particularly, Suda misses the opportunity to add a new dimension to our knowledge of this well-researched and extensively analyzed period by focusing on changes in the party per se.

Similar problems are evident in Suda's treatment of the CPCS as a ruling party. The author describes the major actions party leaders undertook once they came to rule the country, but he does not really analyze how the party rules or how public policy is made and implemented. There is no explicit discussion, for example, of the relationship between party leaders and government bureaucrats or of the ways in which party leaders and members exercise their authority at the regional or local level. Beyond reference to the role of the intellectuals in developments leading to the Prague Spring, the author gives similarly little attention to the party's relations with the intellectuals and specialists who have become increasingly important actors in communist states. Nor does he consider the way in which the Communist party operates in economic enterprises or relations between party members and the broader populace. Given these omissions, the reader gets little sense of the differing problems which face communist leaders once they move from attempting to come to power to establishing a new political and social system to managing an increasingly complex and developed economy. The reader also gains little insight into how these changing tasks, as well as changing world conditions, have affected the party's style of operation, recruitment patterns, or goals at the elite or mass level.

Some of these shortcomings are undoubtedly due partially to the limitations of the sources available to the author. Like other Western scholars, Suda clearly did not have access to the party archives or internal party documents. Instead, he relied chiefly on party congress documents, the popular press, and secondary analysis of works written in Czechoslovakia and the West. He does not appear to have used other available sources, however, including regional newspapers and the party's weekly and theoretical journals, which should contain materials of relevance to a history of the party. Use of such materials might have allowed the author to get below the national level at which most of the story is told.

In sum, the book is useful so far as it goes, but it does not go far enough. As it stands, it is a convenient source of certain kinds of information about the Communist party of Czechoslovakia. One only wishes that Suda had explored some of the broader questions which the history of this party poses for students of Czechoslovak politics, comparative communism, or comparative politics more generally.

SHARON L. WOLCHIK

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Verbände und Staat: Vom Pluralismus zum Korporatismus, Analysen, Positionen, Dokumente. Edited by Ulrich von Alemann and Rolf G. Heinze. (Opladen: Westdeutscher Verlag, 1980. Pp. 266. $12.00, paper.)

Partizipation, Demokratisierung, Mitbestimmung. Edited by Ulrich von Alemann. (Opladen: Westdeutscher Verlag, 1979. Pp. 395. $18.00, paper.)

In 1957 there appeared in *Social Research* a seminal article on the waning of opposition in parliamentary regimes by Otto Kircheimer. In it, Kircheimer noted the dessication of the adversary relationship between both government and opposition, and government and interest groups, and its replacement by the framework of parliamentary arrangements. Ever since, political scientists in Europe and, in particular, Germany, have been examining the "corporate state" and such related topics as "legitimacy," "stagnation of the state' (*Staatsverdrossenheit*), extraparliamentary methods and the "interest-group state." Even today one frequently finds in the German press terms such as "crisis of governance" (*unregierbarkeit*) and "crisis of legitimacy" (*legitimationskrise*).

The best analyses to date of these issues have been brought together by Ulrich von Alemann, the editor of the *German Political Science Review,* in the two volumes under review. The first volume, which is the more important, contains a chapter by Clause Offe in which he updates his theory which in the early 1970s marked the starting point of the corporatist discussion in Germany. Offe had predicted that the postindustrial states would undergo a crisis of legitimacy because these complicated state mechanisms would not succeed in managing the economy in a way that would satisfy the rising expectations of each citizen.

Because Offe's predicted crisis did not materialize (Offe now argues that "moral persuasion" and the lack of trade union militancy made the Germans accept a decline in economic prosperity), von Alemann and others became more interested in the processes and procedures by which governments managed to overcome economic difficulties. Von Alemann's and Heinze's introductory chapter provides an excellent overview of the scholarly literature on corporatism and leads naturally into the two chapters that follow—by Phillippe Schmitter and Gerhard Lehmbruch. Schmitter, in arguments well known to American scholars, again maintains that the key to the understanding of governmental handling of economic crises is the careful analysis of the relations between specialized interest groups and the governmental bureaucracy. Lehmbruch then goes on to point out the differences between the concept of a "liberal" corporatism and the more authoritarian corporate state of a Salazar or Mussolini. He defines liberal corporatism as a mixed system of corporatist elements and democratic interest-group politics that is most appropriate for Western Europe.

The remaining chapters in the book are by representatives of the three major German parties and the president of the German trade union federation (H. O. Vetter), and by a leading industrialist lobbyist (R. Altmann). These chapters clearly indicate that the interest groups feel quite comfortable in dealing with state management structures, provided they can manipulate them. Here we see a major weakness or ambivalence in the discussion of the corporate state. On the one hand the corporate state requires a strong central administration, but on the other hand strong interest groups do not wish to be directed by a strong state. Thus, the liberal corporate state must strike a balance between central economic management and allowing the participation and influence of interest groups. This model seems to work better in smaller countries such as Austria, Sweden, and Holland because the communication networks and participatory mechanisms are less complex. More research is needed on the qualitative differences among modern welfare states. It is incorrect to treat them all, as Offe and von Alemann imply that we should, as similarly liable to crisis and incapable of coping with political dissent. Different countries (with different economic structures and political traditions) respond differently—some on limitations on public sector growth, others with more socialist measures, and still others, like Britain, with political bankruptcy.

Partizipation, Demokratisierung, Mitbestimmung, is a new edition of a previous book edited by von Alemann. It deals with another important issue in German political science literature: that of participation in both its organized forms (co-determination etc.) and its spontaneous form (citizen initiatives). The best chapters examine participation and policy outputs (with special attention to the field of educational policy making) through normative as well as empirical lines of inquiry. The authors of these articles, all young and relatively unknown scholars, question whether the existing participatory patterns of government in the Federal Republic should be preserved or whether alternative models are needed.

Von Alemann has done an excellent job of editing this interdisciplinary book. Of particular note is the treatment of co-determination by Paul Schmitz and Peter Erhardt. They show that under German law many different models of co-determination are being implemented, depending on local circumstances. Most valuable to the American scholar is the bibliography containing 1254 entries of German books and articles dealing with the subjects of participation, democracy, and policy making.

Given the history of German political theories, it is reassuring to see this concern in the Federal Republic with models of participation and pluralistic modes of governance. It reflects the fact that a growing segment of the West German public is ready for more democratic participation than the system now offers. If the parties and interst groups do not respond to these growing participatory needs and demands, the "legitimacy crisis" theory may one day, under conditions of economic crisis and a more conservative leadership, become acutely relevant to the German case.

CHARLES R. FOSTER

Committee on Atlantic Studies

The Emergence of Maoism: Mao Tse-tung, Ch'en Po-ta, and the Search for Chinese Theory 1935-1945. By Raymond F. Wylie. (Stanford, Calif.: Stanford University Press, 1980. Pp. viii + 351. $25.00.)

Raymond F. Wylie has provided scholars of China, political scientists, and lay persons with a timely, well-written, and carefully researched book. The volume deals with the symbiotic relationship between Mao Zedong (Mao Tse-tung) and Chen Boda (Ch'en Po-ta), Mao's political secretary, during the 1935-45 period. The collaboration of these two very dissimilar men resulted in the lionization of Mao in official party history, the "sinification of Marxism," and ultimately produced the cult of Mao Zedong. This book is exceedingly timely; as it rolled off the presses, Chen Boda was being brought to trial, along with the "gang of four," the cult of Mao Zedong was under assault as one symptom of a "feudal and patriarchal mentality," and Beijing's historiographers were busy reassessing Mao's historical role.

The book is organized chronologically, first dealing with the urban intellectual currents of the 1935-37 period and then proceeding to an analysis of the deepening relationship between Mao and Chen after the latter's move to Yenan in mid-1937. Wylie pays particular attention to Chen's contribution to Mao's "sinification of Marxism" and the important Sixth Plenum of the Sixth Central Committee in late 1938 where Mao emerged as the party's leading, if not yet fully mature, theoretical spokesman. In the second half of the volume, Wylie details how Chen and Mao collaborated to create a body of theoretical work which would help legitimate Mao's preeminence and fuel a protracted and calculated campaign to beat political and ideological rivals into submission. By the spring of 1945, "The Thought of Mao Zedong" had been enshrined, at least temporarily, in the party's constitution.

Although the book is replete with new material carefully extracted from primary Chinese sources, several of Wylie's general insights deserve to be underscored. First, many of the interpersonal, policy, and theoretical disputes which animated post-1949 Chinese political life had their origins in the 1935-45 decade, if not before. For instance, Liu Shaoqi (Liu Shao-ch'i) expressed fear of Mao's growing cult as early as 1939 and was lukewarm to Mao's pretensions as late as 1945. Peng Dehuai (P'eng Teh-huai) was similarly reluctant to endorse Mao's ideological infallibility in 1945, despite tremendous pressure to do so.

Second, Wylie charts the growth of the cult of personality, pointing out that it reflected both Mao's manipulation and factors beyond his control during the late 1930s and early 1940s. The most important contributors to the cult's emergence were: the party's own need to create an ideological rival to Chiang Kai-shek, the dissolution of the Comintern in May 1943, and Moscow's recognition of Mao as the principal leader of the Chinese Communist party and acquiescence to his increasingly sweeping ideological claims (pp. 201-04). In a touch of irony, both the Chinese Communist party and the Soviets bear some responsibility for having created a cult that was to so vex them subsequently.

Third, while Mao and Chen Boda were "intellectuals," both recognized that practical political leadership is enhanced by the capacity to shape, if not to monopolize, ideas. Precisely because both men believed in the great power of ideas, they saved a special venom for dissenting thinkers. Mao and Chen periodically identified intellectuals with whom they disagreed, used them as negative examples, and ruthlessly attacked them with virtual impunity—their targets had no organization to shield them. The 1942-43 rectification campaign set a pattern which was to be repeated in 1955, 1957, and through the Cultural Revolution.

Fourth, Wylie persuasively argues that Franz Schurmann's belief that Chinese Communist ideology is divided into "pure" and "practical" components (with "Mao Zedong thought" being the "practical" component) is incorrect. In Wylie's words, "This is mistaken. As I have shown, Mao Tse-tung's thought as it evolved within the party during the years 1935-45 was regarded by its exponents as the sum total of party ideology. In China, Mao Tse-tung's thought *was* Marxism-Leninism" (p. 273). Indeed, it was precisely because Mao's sinified Marxism was Chinese and was a unitary whole that it could provide the basis for a broadly popular nationalist appeal.

Finally, Wylie repeatedly makes the observation that Mao's thought and cult had their greatest appeal among the military. Assuming that this observation is correct, why was it so and does it remain true today? If it does remain true today, what are the implications for the future when Mao's heirs are taking the wrecking ball to his legacy and the military finds itself the fourth priority of the four modernizations?

DAVID M. LAMPTON

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Pakistan in Search of Democracy 1947-77. By Hamid Yusuf. (Lahore, Pakistan: Afrasia Publications, 1980. Pp. xvi + 189. $15.00.)

In this slim volume, Hamid Yusuf analyzes the troubled political history of Pakistan in an attempt to understand why democratic processes and institutions remain alien to Pakistan in spite of the desires of its population and some of its leadership. Yusuf, a former member of the elite civil service of Pakistan, and at present a practicing barrister-at-law, is not a trained scholar but rather a concerned citizen who had attempted to decipher his country's past so as to speculate on its political future. As he indicates in his introduction, "I have . . . concerned myself with the general character of events rather than with their details" (p. xi). The book is not an attempt to apologize for leadership failings, nor an attempt to "blame" external forces (India, the Soviet Union, or the United States) for Pakistan's failure to establish democratic traditions. It is not the first book to concentrate on Pakistan's "search for democracy"—a theme explicit or implicit in most works on government and politics in that country—but it is one of the few works which correctly identifies and analyzes why, after three decades of effort, Pakistan continues to be a "captive" of its most stable institution, the military.

Hamid Yusuf's approach is scholarly but not pedantic. As Lawrence Ziring observes in his foreword, "Hamid Yusuf has written an important, beautifully phrased volume" (p. x). One can readily agree. In fact, Yusuf's style is so good that the many typographic errors in the book do not detract from the analysis. Yusuf relies primarily upon secondary data from the standard works in Englisn (by British, U.S., and some Pakistani scholars) as the basis for his information. The heavy reliance on these sources would lead one to suggest that the increasing amount of material being written in Urdu should have been examined if only to reveal the absence or presence of agreement between 'foreign' observers and those indigenous to the society.

After setting the stage of his thesis with a discussion and analysis of Pakistan's political heritage, Yusuf proceeds along the usual lines of discussing the distinct political phases the country has gone through. Hence, he examines the "parliamentary period" from 1947 to 1958 as the "first experiment" in democracy; he analyzes the dictatorship of Ayub Khan as an experiment in "controlled democracy"; he views the second military government of Yahya Khan as an interlude marked by civil strife and the secession of East Pakistan, and the Bhutto period as "the return of the politicians." He offers little insight into the present military regime (since the book was published in Pakistan and government censorship exists there, there are very practical reasons for this omission). He does, however, offer an epilogue which combines a conclusion with a general forecast of what might occur in the immediate future.

Although it was not his intention to provide details of the events covered, using nonscholarly accounts (one suspects personal interviews) and his knowledge of the political system gained from his experience as a civil officer, Yusuf does provide insights regarding the motives behind the actions of leaders of the various periods and how they sought to maneuver so as to maintain and extend their power over others. The manipulations of Iskander Mirza (the last governor-general and first president, 1956-58) are revealed in detail as are the reasons for President Ayub's and Prime Minister Bhutto's failure to achieve their publicly stated objectives.

One drawback is that Yusuf tends to be slightly intimidated by "established" viewpoints. In his analysis he often defers to the works of established scholars with only a hint that his own analysis leads to opposite conclusions. For example, when he analyzes the "collusion" between General Peerzada and Bhutto in stimulating the mass agitation which led to Ayub's fall, he cites G. W. Choudhury's analysis, but in a footnote he reminds the reader, "It may, however, be borne in mind, that G. W. Choudhury was a member of the Yahya Government which all the politicians, including Bhutto, desired to topple" (p. 95). Consequently, Yusuf's own conclusions tend to be muted in the process.

Although all of the chapters are of uniformly high quality, the last two, analyzing the Bhutto period and summarizing the book, are the best. There are several reasons which support this assertion. Few analyses of the Bhutto period have been objective although most claim to be unbiased accounts. Yusuf's analysis comes very close to being dispassionate. He details the pluses and minuses of the 1971-77 period and gives Bhutto credit for his inspirational leadership. Bhutto's leadership qualities are balanced, negatively, with his intolerance of opposition and his desire for complete political control. Without providing the detail of analysis found in the excellent book by S. J. Burki (*Pakistan Under Bhutto, 1971-77,* reviewed earlier in the *Review*), Yusuf's overall assessment of Bhutto and his critical period of leadership agrees substantially with Burki's. Yusuf's epilogue, while more optimistic than the facts might support, is a very good summation of his case for democracy in Pakistan and the prospects for its democratization. He maintains, "The faith and resilience, patriotism and a capacity for sacrifice in the nation inspire the hope that the emerging leadership will set the country forward again in quest of harmony and democratic existence, of justice and genuine growth, which will lead and not drive the people and by a wise policy lay the foundation of a happier future" (p. 184).

On balance, Yusuf's work is a critical but sympathetic examination of Pakistan over its 30 years of existence. The fact that it was written not by a scholar but by a civil servant turned lawyer makes it even more impressive. As a primer for all those interested in Pakistan, it is one of the best available. Unfortunately, the publisher does not have outlets in the U.S. or Great Britain so that it will not have as wide an audience as it deserves.

ROBERT LAPORTE, JR.

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International Politics

Syria and the Lebanese Crisis. By Adeed I. Dawisha. (New York: St. Martin's Press, 1980. Pp. xii + 208. $25.00.)

This study is an attempt at systematic treatment of the Syrian decision-making process and environment during the Lebanese civil war of 1975-76. It was done in association with the International Crisis Behavior (ICB) project, directed by Michael Brecher. Much of the research was based on interviews with Syrian officials intimately involved in those initial decisions. It is, therefore, a commendable effort and a good start in the search for a detailed and objective study of crisis situations in the foreign policies of the Arab Middle East.

There is no question that the careful presentation of the various critical decisions, setting out the values and attitudes of the main decision makers, as well as their perceptions of the principal actors, provides a clearer, if somewhat repetitious, account of the crises and how they were handled. However, it is difficult at times to avoid feeling that the conclusions are, in fact, the original assumptions. Thus much of the background material in part 1 sets the stage for the assumptions and interpretations that are made in discussing the decisions and the environment.

Adeed Dawisha accepts at face value the various pronouncements of the Syrian leaders—both the ones made publicly as well as those given in private interviews. Thus, Dawisha argues that Syrian leaders had a bad image of the United States, even when the U.S. endorsed Syria's military intervention in Lebanon. The author claims that this bad-faith model is "engrained deeply in the Syrian psyche as a result of years of Syrian-American hostility" (p. 107). This may be the case (although it is by no means proved), but an alternate explanation is possible. Perhaps the Syrian leaders felt that they *had* to declare their anti-imperialist stance at a time when they appeared to be on the side of imperialism (the U.S.) and against the left. To have done otherwise would have alienated many of their leftist followers.

One major motivation or concern to the Syrian leaders is not listed as a constituent element of their "attitudinal prism" (pp. 106, 169), namely, personal and political survival. While this concern was not voiced by the Syrian decision makers, one can argue that this was, indeed, one of the most important factors influencing their decisions. To the extent that this unarticulated value was not fully considered, the study is incomplete or somewhat deficient.

The book tends to be sympathetic to the Syrian leaders, if only because Dawisha accepts their statements and viewpoints without challenge. In the process, those opposing them appear to be either misguided or naive. The Syrians backed the rightist element in Lebanon, we are told, because they perceived it to be "committed to the preservation of Lebanese unity" (p. 170). But surely this assumption is subject to much dispute. Was the progressive left opposed to Lebanese unity? How committed was the right to Lebanese unity? Indeed, Dawisha himself writes in the epilogue that the "Christian Maronite leadership . . . proceeded with their plans to effect a *de facto* partitioning of the country" (pp. 186-87). Therefore, either the Syrian leaders were mistaken about the right wing's commitment to Lebanese unity or, more likely, their main concern was not unity per se, but the possible ascendancy of the left in Lebanon. In general, as the author states elsewhere, the Syrians wanted to see a balance of power in Lebanon where neither of the two protagonists could completely defeat the other.

Dawisha's study is a useful addition to the literature in that it makes an important contribution to the systematic and detailed analysis of a major foreign policy issue.

MICHAEL W. SULEIMAN

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The Soviet Union in the Third World: Successes and Failures. Edited by Robert H. Donaldson. (Boulder, Colo.: Westview Press, 1981. Pp. xiv + 458. $25.00, cloth; $12.00, paper.)

Alarmist views abound these days about a Soviet blueprint for the control of the Third World and about the Kremlin's ability to exploit the instabilities of those countries to its own advantage. It is therefore salutary to have a sober analysis of what the Soviets have actually been able to attain in the former colonial preserves of the Western powers. And the appearance of such a book is opportune for another reason. Twenty-five years have elapsed since the Kremlin reversed its policy of berating the middle-class regimes of the newly independent nations, and started counting statesmen like Nehru or Nkrumah as gallant leaders of their countries' struggles for meaningful political and full economic independence.

During these 25 years much has changed in Soviet policies, and the stance and situation of the Third World have altered much too. Yet many scholars and publicists still tend to argue as though Khrushchev's boastful claims that the USSR will "bury the capitalist world" by depriving it of the diplomatic support and raw material resources of the former colonies were still the Kremlin's operational code. Similarly, they ignore the fact that the Third World is no longer (if it ever was) a tabula rasa on which the Soviets could inscribe any pattern they wished.

This informative and balanced symposium evolved from a conference held in late 1979 at the U.S. Army War College under the auspices of the Strategic Studies Institute, and attended by 20 leading experts with academic or intelligence research backgrounds. The book of 21 chapters is conveniently divided into five parts. Three deal with geographic areas: Latin America, Asia, Africa and the Middle East. One section discusses the context and the instruments of Soviet policy, and the concluding section draws up a balance sheet of Soviet operations in the Third World. Some 32 tables provide often hard-to-get information on Soviet assistance, trade, arms transfers, naval operations, diplomatic peregrinations, technical and military personnel, and academic exchanges. The end notes to many chapters are also most useful.

Among the chief virtues of the book is the effort of its contributors not to rest content with describing the extent of Soviet penetration but to come to grips with the actual results of that presence. In other words, they cope with defining and assessing that elusive concept of "influence." The prevailing tone of the contributions is that one cannot mechanically equate presence with influence, and they often pinpoint what elements in the situation make it impossible or counterproductive for the USSR to attempt or to attain total control.

In this respect, the theoretical framework of this symposium is representative of advances that have taken place in the field in recent years. It demonstrates to what extent specialists now resort to a careful "cost-benefit" analysis, finding the former "zero-sum" approach no longer applicable. Robert Donaldson is to be congratulated for focusing the symposium on this crucial aspect of Soviet-Third World relations. To one extent or another, all authors argue that despite the seemingly brazen and unhampered Soviet expansion in the Third World, Moscow increasingly faces a variety of obstacles and burdens that limit its ability as well as its willingness to seek exclusive economic, political or strategic advantage. Moreover, they stress that the Soviets have rarely been able to compel a Third World government to adopt a policy that it was not inclined to pursue anyway. These findings suggest a number of prescriptions for American policy—mainly, that its focus should be more on the internal conditions of the Third World and less on the Soviet connection.

However, in appreciating the care with which the contributors analyze the meaning and actual operation of such elusive yet highly charged words as "influence" or "presence," it is unfortunate that more care was not taken with the definition of other terms that carry similar emotional connotation. This is especially striking with the imprecise use of the word "Communist." Thus, the late President Allende of Chile is described as "the first Communist to be elected to power." Or, in one place the ruling parties of Angola and Mozambique are described as being outright "Communist," and in another as merely "pro Marxist-Leninist." Greater accuracy and consistency in this respect would have enhanced the points the book so successfully conveys by carefully analyzing what actually lies behind the common assumptions that feed our apprehensions about Soviet activities, aims and designs.

ELIZABETH KRIDL VALKENIER

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China Under Threat: The Politics of Strategy and Diplomacy. By Melvin Gurtov and Byong-Moo Hwang. (Baltimore, Md.: The Johns Hopkins University Press, 1981. Pp. xi + 336. $22.95.)

The existence of linkages between domestic and international politics is fairly well accepted in comparative foreign policy studies, although the nature and extent of those linkages are still subject to debate. Yet it has only been in very recent years that political scientists have moved away from a simplistic model of Chinese politics and toward more sophisticated analyses of political developments in one of the world's oldest existing political systems.

Melvin Gurtov and Byong-Moo Hwang individually examine five instances in which the People's Republic of China became militarily involved with a foreign adversary along its borders. This study examines the events and decisions leading up to the Korean War (1950), the Taiwan Straits crisis (1958), the Sino-Indian border war (1962), involvement in the Vietnam conflict (1965), and the Sino-Soviet border clash (1969). Gurtov wrote the introduction, the Taiwan Straits case—which was originally published in *Modern China* (Jan., 1976)—the conclusion, and co-authored with Harry Harding the discussion of the Vietnam case—which was a 1971 RAND Corporation report. Hwang authored the other three.

The authors examine the domestic and international policy settings and the specific relationships between domestic developments in China and foreign policy perceptions and behavior. This is done in part through the construction of a theoretical framework which develops a set of propositions which support their basic hypothesis that China's foreign policy "serves domestic political values institutions and programs" and is primarily defensive (p. 17).

Chinese intervention in Korea was neither planned nor sought, but resulted from the leadership's perception of "imminent" danger to China's national security (p. 56). The American-led drive toward the Yalu River triggered the Chinese decision, but the method of intervention was closely tied to domestic considerations.

Gurtov quite convincingly develops the thesis that the Chinese government initiated the Taiwan

Straits crisis to counter a perceived external threat. American anti-Chinese activities in Asia coupled with the dispatch of American troops to Lebanon convinced Mao Zedong and other Chinese leaders of an increasing American threat. To counter this, the Chinese adopted a low-risk preemptive policy of shelling the offshore islands of Quemoy and Matsu in hopes of deflecting the confrontation from the military to the diplomatic arena. In this they succeeded but only after initial American overreaction.

The Sino-Indian border crisis further substantiates the existence of linkages between domestic developments and foreign policy behavior, which was true in this instance for both participants. As in Korea, the Chinese first sought diplomatic means to resolve the apparent crisis, but when that failed, they again resorted to military force. In this case the nature of the response was closely linked to domestic considerations.

The Vietnam study is somewhat different from the others since it is an analysis of the 1965 purge of Luo Ruiqing, head of the Chinese armed forces. Gurtov and Hwang argue that Luo's purge had less to do with the escalating conflict in Vietnam than it did with the implications of China's strategic military doctrine for domestic policies.

A Soviet military buildup along China's border and other Soviet incidents, combined with the 1968 Soviet invasion of Czechoslovakia, raised fears in the minds of China's leaders of a possible Soviet attack. Domestic weakness resulting from the Cultural Revolution, coupled with this new threat, resulted in another instance in which China used limited military force to preempt the situation and thereby prevent it from escalating into a major confrontation.

Although there was substantial internal debate over the specific foreign policies adopted in each of these cases, they all illustrate a consistent pattern of behavior with both prior and subsequent linkages between domestic politics and foreign policy initiatives. One weakness of this study is its failure to link each case study explicitly with the propositions developed in the introduction. Nor are such links made explicit in the conclusion. The case studies are excellent analyses, but the theoretical framework is not fully developed nor consistently used. It appears to be an after-the-fact effort to join the cases together.

With that reservation aside, the analyses convincingly buttress the argument that China's foreign policy is not as irrational, ideological or as deterministic as some have argued. This study complements Whiting's *The Chinese Calculus of Deterrence* (1975) and Kim's *China, the United Nations and World Order* (1979) which similarly conclude that although Chinese foreign policy has some unique features, it is influenced by some of the same variables and developments that influence other nations.

FRANK E. ROGERS

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National Power and the Structure of Foreign Trade. By Albert O. Hirschman. (Berkeley: University of California Press, 1981. Pp. xxii + 172. $16.50.)

Albert Hirschman's landmark study of foreign trade as an instrument of statecaft first appeared in 1945 (price $3!). Writing against a background of debates about German economic "aggression," protectionism versus free trade, and state trading versus private enterprise, Hirschman addressed the question of the political nature of international trade. Was trade an instrument of national power? How could some states use trade to exert leverage on other states? Could trade be depoliticized?

Hirschman identified two processes by which trade can affect a state's power: the "supply effect," which refers to the effect on a country's power position of imported goods and services, and the more subtle "influence effect," which is based on the ability of any sovereign state to interrupt its own export and import trade. In a world of sovereign states, he argued, international trade has an inherently political dimension based on these two effects. Noting that the classical economists' concept of the "gain from trade for any country is indeed nothing but another expression for the total impoverishment which would be inflicted upon it by a stoppage of trade" (p. 18), he called attention to the inextricable link between trade dependence and gains from trade. The more a country benefits from trade, the more dependent it is on trade.

Well and good, one might say; but why review this book again 35 years after it was first reviewed in the *APSR*? First, the new edition contains a new preface in which the author describes himself as the "founding grandfather" of *dependencia* theory and proceeds to suggest that both he and the *dependencia* theorists may have overlooked some effects of asymmetrical dependency that work in favor of small dependent countries in their dealings with large dominant countries. A country that is dominated by ties to a large and rich country is "likely to devote its attention with single-minded concentration to this uncomfortable situation and to attempt to loosen or cut these ties," Hirschman argues, but the dominant country is likely to be preoccupied with its relations with other, more important, countries (p.

ix). Thus, the economic disparity generates a psychological disparity that mitigates the effects of economic dependence.

A second reason for reviewing this book is to call the attention of scholars, especially political scientists, to an important, relevant, and often neglected book. Although much has been written about dependence and interdependence in the last decade or so, this work was often carried on in apparent ignorance of Hirschman's pioneering book. It is ironic that this book should have been so ignored during the 1970s, when dependence and interdependence became academically stylish terms. Many graduate students trained in the 1970s seem to think that Latin American scholars invented the concept of dependence and that Richard Cooper's *Economics of Interdependence* (1968) was the first serious scholarly treatment of the subject. The reissue of Hirschman's book will not only correct these mistaken impressions, it will also clarify much of the muddy conceptualization so prevalent in the last decade. Hirschman's work is clear, concise, judicious, objective, and respectful both of the facts and of the interpretations by other scholars. It is, in short, a model piece of scholarship.

The contemporary international political economist or student of international politics should not be misled by the age of this book; it is as relevant to today's problems as it was to those of the 1940s—indeed, it is more so. This book belongs in the library of every serious international political economist, and its republication is most welcome.

DAVID A. BALDWIN

Dartmouth College

Change in the International System. Edited by Ole R. Holsti, Randolph M. Siverson and Alexander L. George. (Boulder, Colo.: Westview Press, 1980. Pp. xxxii + 316. $28.75, cloth; $12.50, paper.)

This volume of eleven essays dedicated to Robert C. North is divided into three parts. The first considers problems in describing international systems and, particularly, the paucity of attention paid to system dynamics. The second part focuses upon the sources of system change. The concluding section analyzes national constraints upon system change from the perspective of American foreign policy. Although the editors' introduction claims considerably more thematic unity for the essays than is in fact there, they must be applauded for having assembled a remarkably literate set of papers by a remarkably talented galaxy of authors. They have honored Bob North in a manner befitting his extraordinary contribution to our discipline. The volume should be *de rigeur* for scholars and graduate students interested in the "state of the art" of research on international systems.

It is to the editors' credit that they did *not* seek to impose too disciplined a framework upon their authors. As a result, the volume reveals the rich diversity of approaches, ideas, and methodologies that are being employed to study international systems as well as the lack of paradigmatic consensus that characterizes the discipline in this area.

Despite the diversity of topics addressed, a number of key themes emerge. Fundamental questions concerning the definition of international systems and structure and their properties remain unresolved. Dina Zinnes' initial observations concerning the lack of progress in international systems analysis since the 1960s and the relative inattention to system dynamics are largely reinforced by the essays that follow.

A number of the essays are quite complementary. Nazli Choucri's splendid analysis of how to combine insights from economic theory and international relations theory in order to apprehend international system change is reinforced by Robert Keohane's lucid assessment of the validity of the theory that strong international economic regimes depend on hegemonic power. Although Keohane's investigation of international regimes fails to disconfirm the theory, analysis clearly reveals the theory's explanatory limitations.

Complementary, too, are the essays by P. Terrence Hoppmann and Timothy King, on the one hand, and Charles Hermann and Robert Mason, on the other. Both apply innovative methodologies and sources of data to assessing the role of crises in international systems transformation. Using as their data the results of a content analysis of transcripts of negotiations to which they then apply a quasi-experimental design, Hoppmann and King go some distance toward confirming the seminal importance of the Cuban missile crisis and the partial nuclear test ban treaty in the evolution of cold war to detente. For their part, Hermann and Mason use CREON event data to determine the characteristics of events that forecast the onset of international crises. The question they address is of fundamental importance, but limitations in their data base and the scrupulousness with which they treat their data preclude their reaching any determinant conclusions.

Finally, the essays by Alexander George, and Ole Holsti and James Rosenau, among the most illuminating in the volume, inform one another and paint a composite picture of the problems confronting policy makers in the United States in their efforts to produce coherent and consistent foreign policy. George describes the normative and cognitive bases for "policy legitimacy" as reflected in Roosevelt's attempt to develop his "Grand Design" and in the effort of Nixon and Kissinger to reorient U.S.-Soviet relations, and he explains why both failed to achieve legitimacy at home. Holsti and Rosenau use an extensive survey of U.S. leadership opinion to describe the extent to which cold war axioms have given way to post-Vietnam axioms. The results of their analysis dramatize the contemporary dissensus over foreign policy in this country and leave the reader pessimistic about the prospects for policy legitimacy in the near future.

Although space limitations preclude detailed discussion of the other essays, in this volume, they, too, are informative and stimulating. K. J. Holsti's argument that contemporary observers of international politics, in their enthusiasm for "interdependence," have failed to appreciate the sources and signs of fragmentation and disintegration in the global system is a refreshing new look at an old question. Stephen Genco's criticism of integration theory and his analysis of the role of crises and system transformation in regional integration helps to explain existing flaws in that theory. Randolph Siverson argues persuasively, though not conclusively, that certain wars fundamentally alter international systems and that the earlier conclusions of Organski and Kugler are by no means the last word on this issue. Finally, Volker Rittberger illustrates how the transfer of modern technology and science to the Third World may impede the solution of the very problems they are intended to overcome.

The range and diversity of essays in this volume and the wealth of conceptual and methodological insights they encompass assure it an important place in the literature on contemporary international relations.

RICHARD W. MANSBACH

Rutgers University

Political Psychology and Biopolitics: Assessing and Predicting Elite Behavior in Foreign Policy Crises. By Gerald W. Hopple. (Boulder, Colo.: Westview Press, 1980. Pp. xiv + 218. $19.50.)

There has been much ado about idiosyncratic variables in foreign policy decision making during the last 15 years. In a tour de force, Hopple provides a state-of-the-art review of the research on the personality traits, styles, beliefs, values, and other idiosyncratic characteristics of political decision makers. He compiles all the conventional wisdom on methodological and conceptual problems experienced in such studies, and he adds useful insights of his own.

Two major chapters are essentially literature reviews, which will be valuable to initiates in the field. One focuses on biopolitical foundations and the other on psychological foundations of elite foreign policy behavior. By far the most interesting portions of the book are chapters 4 and 5 in which Hopple reports a massive and provocative project of his own in testing the relationship of elite values and foreign policy behavior.

His research draws heavily from the work of Milton Rokeach. Using a content analytic approach, Hopple develops value profiles of the foreign policy elites of 39 countries, out of an original group of 56 "which had initiated forty or more foreign policy events during the time span from 1966 to 1969, according to the World Event Interaction Survey (WEIS) events data set" (p. 120). The source documents for the value profiles were derived from the *Daily Report* of the U.S. Foreign Broadcast Information Service (FBIS).

He then used a variety of statistical manipulations, particularly regression analysis, in a research design of four independent variables, actually clusters of variables (psychological, societal, interstate, global), four intervening variables (Western, closed, unstable, Third World groups of states), and three dependent variables (constructive diplomatic behavior sent, nonmilitary conflict sent, force sent).

Hopple's extensive data support the argument that all source variable domains—internal and external, individual, state, international levels—should be operationalized in foreign policy analysis. His data also support a number of long-held assumptions, e.g., that the individual-level factors are of more potency the higher the level of the decision maker in the hierarchy, the less routine the context, and in closed or developing states as contrasted with the open and developed states. Moreover, he finds evidence of homogeneity among foreign policy elites across countries, which is consistent with Rokeach's view that institutions, such as science and religion, possess value patterns unique to themselves but similar across national boundaries.

There are deficiencies in Hopple's data, which could be eliminated in a fresh replication. For example, the number of decision makers taken to represent the foreign policy elite of each country could be increased. (Hopple used president and foreign minister.) The defect, acknowledged by Hopple, of "pooled" data in his regression analysis could be avoided in a more rigorous effort that relates data on values to behavior which clearly came later. The level of inter-coder reliability could be higher. (Hopple's was .83, significant at the .001 level.) Any bias in the documents selected for content analysis could be reduced. (Hopple was dependent on FBIS officials.) Nonetheless, the cumulative effect of these and other concerns is to downgrade Hopple's findings from "firm" to "suggestive." Yet more than that most such studies, Hopple's deserves replication.

The works of others which he reviewed, as well as Hopple's own project, reveal how central content analysis has been to investigations in this field. Rather than a conceptual breakthrough, our central need may be for a technological breakthrough which would permit more numerous and standardized content analytic studies. One can also project that at some point in the future the VSA (voice stress analysis) approach of T. C. Wiegele, which Hopple notes in chapter 2, may be used frequently to enhance the validity of content analytic data. It offers the advantages of being nonreactive and not requiring the cooperation of the subject.

Finally, one can quarrel with the title of the book, which one suspects the publisher crafted in an effort to be trendy. The discussion of the biopolitical literature could have been omitted with little loss, which may simply testify to the infancy of that field and its limited contributions to date. Further, crisis decision making is not a central concern of the book. In his own research project Hopple defines crisis decision making as that which leads to the use of force. Yet his research design focused on three behavior outcomes, dependent variables noted above, with no special attention to force.

JOEL E. ANDERSON, JR.

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Arabia, the Gulf and the West. By J. B. Kelly. (New York: Basic Books, 1980. Pp. ix + 530. $25.00.)

Many studies written on political subjects in recent years substitute dullness for scholarship, figures for facts, jargon for analysis, purported science for apparent opinion, caution for courage. Those who have become accustomed to the standard pallid fare will find this volume startling in method and style as well as in content and philosophy. J. B. Kelly is relentlessly polemical, delightfully opinionated, entertaining and unconventional, brutally honest. Yet, the depth of his knowledge and the power of his presentation promise to revolutionize the American approach to the Middle East and the Persian Gulf. This volume contains important messages for students of international relations, U.S. foreign policy, and the international political economy. Many will disagree with one or more of Kelly's conclusions, but no one involved in any of these fields can ignore Kelly's epic achievement if he hopes to address the critical issues affecting the reasons for the shift in power between the industrialized nations of the West and the oil producers of the Persian Gulf.

Kelly is an historian who previously published *Eastern Arabian Frontiers* and *Britain and the Persian Gulf, 1795-1880* and who has spent more than 25 years traveling, working, and studying the Gulf. His volume lacks the analytical categories that would often denote a political scientist's *modus operandi.* Even his chapter titles entail literary allusions that all but the *cognoscenti* will have difficulty following. Instead, the reader is treated to a unique and comprehensive intellectual panorama of the social and political history of the major states of the Persian Gulf and the reasons why the Western positions in the region disintegrated. Kelly explores the history of the development of South Yemen, Oman, the emirates, Iraq, Saudi Arabia, Iran as we know them today; he examines the British withdrawal from the area, the OPEC price rises, the altered relationship between oil producers and consumers, the emerging roles of the Soviet Union and the United States in the Gulf.

His themes are multitudinous: (1) The states of the Persian Gulf represent fragile and unstable regimes ruling over primitive and tribal societies only recently exposed to the West. (2) These countries are plagued by the internal corruption, the culture shock, the economic dislocation, and the social upheavals caused by sudden confrontation with the West and the simultaneous receipt of billions in petrodollars. (3) Western firms and governments fawning over these regimes with arms sales, gargantuan construction projects, and economic "special relationships" have only worsened the inherent instability and fragility of these regimes. (4) The oil producers have been viewed incorrectly in the West as deserving beneficiaries of massive oil royalties instead of being recognized as greedy, venal, arrogant and corrupt leaders who will squeeze the West while also exploiting the oil-poor developing countries who are the real victims of the energy crisis. (5) Both the British and the oil companies provided essential benefits to a region which cannot control its own affairs in a stable manner without a strong outside force. (6) In return, the Islamic peoples of the area maintain a deep hatred of the Christian West, which leads them constantly to break agreements and to attempt to seek whatever riches can be squeezed from the willing and weakened Western leaders. (7) Relations among peoples in the region are filled with deeply held animosities and antagonisms; Iran and Saudi Arabia, for example, were never the twin pillars of the area but are rather the "twin bullies" over the smaller and often weak sheikdoms. Kelly also sees dangers to the smaller states of the Gulf from Iraq, Russian-supported subversion and Marxist-Leninist radicals. (8) Consistent with their competition for four decades, American oil companies helped to undermine the British position in the area only to realize too late that they had thereby also compromised their own security (for instance, the Saudis and the CIA aided the Omani rebels in 1958 against the British-backed sultanate). (9) A stronger and more unified position by both the companies and the Western governments in the early stages of the OPEC revolt in 1970-1971 would have stopped the incipient rebellion in its tracks, especially if it had not been accompanied by the announced British withdrawal from the area. (10) The October 1973 war was only one stage in the process of the OPEC revolution; it was neither the culmination nor the origin of that revolt.

These themes are molded in a theoretical and methodological context significant for students of international politics, American foreign policy, and the Middle East. Kelly maintains that the international politics of the area (and implicitly the globe) cannot be understood without reference to the historical antecedents of contemporary disputes and enmities. He thereby reduces the importance of individual leaders and even regimes, while stressing national culture, character, a people's collective memory, and geopolitics. Contemporary trends are perceived through glasses tinted by the past, granting different meaning to the stated aspirations of individual governments than might otherwise be the case if this history were not understood.

This approach challenges both the school which emphasizes bureaucratic decision making as well as that which views international relations as a network of interacting billiard balls devoid of substantive reality. It should also be disturbing to students of American foreign policy, especially yesterday's experts on Indochina who are today's Mideast savants. As Kelly demonstrates by example, foreign policy cannot be made by "quick study." Without an initial effort to comprehend the historical context of the international politics of an area, policy makers and learned observers risk approaching a region in terms of images gained in carefully abridged briefings or in staged meetings with foreign leaders. As for students of the Middle East, Kelly challenges accepted American images of Saudi and Iranian politics, Persian Gulf regional relations, "progressive" movements, the role of the British, and the origins of the energy crisis. By contrast with the American study of the region, Kelly's study is distinctive for the absence of the Arab-Israeli conflict as a central focus.

Some may view this volume as the call of an ideological conservative for a neo-imperialistic reemergence in the West, Rudyard Kipling style. Yet, Kelly's perspective is far more complex. He advocates many positions usually maintained by liberal writers: he is sharply critical of the shah, opposed to huge arms sales, and concerned about OPEC oppression of the Third World. He does not call for a massive Western reentry into the area, but rather a selective focus on strategic locations such as Oman and South Yemen—now under heavy Russian influence. A careful reading of this volume reveals an intimate and impressive knowledge of the region and an intricate series of perspectives, forthrightly and elegantly written, which taken together challenge accepted concepts about international relations, American foreign policy, and the Middle East.

Kelly's volume cannot be ignored by anyone seriously interested in any of these fields of study.

STEVEN L. SPIEGEL

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The Soviet Union in World Politics. Edited by Kurt London. (Boulder, Colo.: Westview Press, 1980. Pp. xiii + 380. $27.50.)

This book is essentially a collection of essays on Soviet foreign policy by a variety of distinguished specialists. The first section of the book describes itself as a "Conceptual Appraisal" of Soviet foreign policy. In two essays by Leonard Schapiro and Adam Ulam, respectively, the work attempts to provide a general overview of Soviet foreign policy, its goals and strategies, as a prelude to nine regional case studies and an overview of Soviet military power. The concluding chapter attempts to summarize and draw conclusions from the ten studies. Among the other contributors to the work are such well-known scholars as Harold Hinton, Alvin Rubinstein and Hugh Seton-Watson, to name only a few.

The work is designed for an audience of advanced students of international relations and Soviet foreign policy and probably well-read members of the public. There is not likely to be much new in this work for the scholar specialized in Soviet politics or foreign policy.

The work purports to study both Soviet "tactics and goals" (p. 358) and to examine basic Soviet strategy in foreign policy (p. ix). To some extent it has succeeded, but not totally so. Although the two introductory, conceptual essays are excellent in and of themselves, they do not provide an adequate analytical framework for the entire work. The Schapiro essay focuses on the relevance of the totalitarian model in foreign policy, while the Ulam essay presents an historical overview of Soviet behavior in the international arena since 1917. There are no new theories or interpretations in either essay. What is missing, also, is an essay setting the stage for the nine regional case studies which follow.

Each of the case studies is well written and presents a concise overview of Soviet foreign policy in a given region since World War II. However, there is nothing in section 2 (pp. 55-290), to integrate those essays. The order of the essays itself may be questioned, and there are no introductory essays giving an overview of larger regions. For example, there are three chapters on Soviet Policy in Asia: East Asia, Southeast Asia and South Asia. It would make sense to have a brief introductory essay describing possible similarities and linkages among policy in those three areas. Soviet policy towards Western Europe and towards the United States can be found in separate chapters in different parts of the book. Yet there are enough common patterns in Soviet relations toward the West to warrant some sort of introductory essay linking the two chapters.

The final essay by London is an attempt to present "Summaries and Conclusions." He perhaps is more successful in the former task than in the latter. The chapter summarizes each of the preceding essays well, but perhaps so detailed a synopsis denigrates the reader, who surely could glean the major points for himself. Moreover, the final chapter frequently includes very long quotations from the earlier essays, which seems unnecessary. There is an attempt to integrate the findings of the several authors, but the analysis is sprinkled with numerous "but's" and "however's." A more substantial integrative analysis in the final chapter, as well as brief integrative essays at appropriate points in the book, would have much improved this collection.

On balance, this book is a worthwhile acquisition for a college library and good reading for the advanced student of international relations or for the well-informed reading public. It does not break any new ground in the study of Soviet foreign policy, but it does present clear, historical synopses of recent Soviet policies in the major regions of the world within one volume. It thus is a useful reference work for the student of post-World War II foreign policy.

NORMA C. NOONAN

Augsburg College

Iran, Saudi Arabia, and the Law of the Sea; Political Interaction and Legal Development in the Persian Gulf. By Charles G. MacDonald. (Westport, Conn.: Greenwood Press, 1980. Pp. xv + 226. $28.50.)

The Third United Nations Conference on the Law of the Sea, after some 12 years of effort, has still not settled on a method of delimiting maritime boundaries between states whose continental shelves and/or exclusive economic zones are either adjacent to one another or overlap. This problem, however, has to a considerable extent been resolved in the Persian Gulf. MacDonald explains how this was done and considers other relevant aspects of the Law of the Sea as developed and applied in the Gulf.

The introduction is an essay entitled "The Role of Developing States in the Law of the Sea as a Major Public Order Concern." It establishes the context and approach of the book and touches upon many matters relevant to the subject but not discussed in detail. This allays, to some extent at least, possible criticisms about omissions. Chapter 1, "The Persian Gulf Context," gives a brief geographical and historical background and a fuller treatment of both contemporary international relations in the Gulf area and the individual foreign relations of Iran and Saudi Arabia. There are errors, such as attributing British withdrawal from the area to "a resurgence of nationalism in the Persian Gulf" without mentioning the British policy of withdrawal from nearly all positions "east of Suez" (p. 24), and attributing the settlement of the Bahrain dispute to Iran without mentioning either the Saudi desire to take over the island or the United Nations-sponsored plebiscite which overwhelmingly opted for independence (p. 33), but on the whole the chapter does provide a satisfactory background.

Chapters 2 through 4 cover in substantial detail the major maritime conflicts in the area, dividing them into disputes over maritime space and those over exploitation of the sea's resources, and considering the Iranian, Saudi and community (Gulf) viewpoints. In general, the handling of these complex matters is adroit and satisfying. Chapter 5, on regional efforts at pollution control, is only six pages long, including notes, and is clearly an introduction to a topic which deserves further attention. Chapter 6 covers the roles of Iran and Saudi Arabia at the three UN conferences on the law of the sea, based largely on published conference documents. Here, interviews with people familiar with the unpublished story would have been most helpful, for it is likely to have been even more interesting and important.

The final chapter, "The Roles of Iran and Saudi Arabia in the Development of the Law of the Sea," is only eight pages long, but it contains much useful material and some thoughtful analysis. MacDonald makes four important points: (1) There is little regional cooperation in marine affairs in the Gulf except to some extent in pollution control; (2) Iran and Saudi Arabia have adapted and applied specific legal concepts in settling their maritime disputes and in particular have strengthened the concept of "equitable principles" as the basis for delimiting maritime boundaries between proximate states; (3) "Considering the growing vulnerability of semi-enclosed seas to increased usage, the necessity of establishing a special regime for the Persian Gulf continues to be cited" (pp. 203-04); and (4) "The approaches of developing states to international law are similar to those of advanced states" (p. 205).

Faults can be cited; e.g., there is some unnecessary repetition within the text; the oversimplified maps lack legends explaining their symbols; inadequate attention is paid to the influence of outside forces (the U.S.A., the USSR, Japan, the Group of 77, the Arab League, etc.); and others; but on the whole the book is well written, amply documented and a fine reference tool. I look forward to a sequel in about ten years' time.

MARTIN IRA GLASSNER

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The Official History of Colonial Development: Vol. 1, **The Origins of British Aid Policy, 1924-1945;** Vol. 2, **Developing British Colonial Resources, 1945-1951;** Vol. 3, **A Reassessment of British Aid Policy, 1951-1965;** Vol. 4, **Changes in British Aid Policy, 1951-1970;** Vol. 5, **Guidance Towards Self-Government in British Colonies, 1941-1971.** By D. J. Morgan. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xxx + 253, xiii + 398, xvii + 334, xv + 275, xvii + 382. $37.50, each.)

An *official* history this certainly is: recounting bureaucratic concerns during the years that Britain transformed its colonial policy into development policy, filtering out most of the wider debate in British politics, offering the classic official view: discreet, centrist, unlikely to offend. The tempered prose and measured judgments become even more cautious as D. J. Morgan moves from the beginning of his story in the 1920s to the present.

Despite the author's self-conscious forswearing of a point of view and the virtual absence of any analytical or theoretical insight, a number of themes with contemporary resonance do appear in this lengthy account. From the first timid steps away from strict colonial self-sufficiency in the 1920s to the final absorption of the colonial bureaucracy into the development and foreign affairs ministries, the definition of development itself and especially economic development, was debated. During the interwar period, British civil servants and politicians staked out the lines of argument between proponents of "basic human needs" and those intent on a more conventional industrialization strategy. Lord Lugard's opposition to higher education as the source of a deracinated political class pervaded official attitudes before World War II, but by 1937, Earl De La Warr urged "not the investment of large sums of capital, but the improvement of human material" (Vol. 1, p. 59). On the opposing side, a skeptical Colonial Office education adviser:

> Suppose you have a completely successful scheme of mass education—everybody eating fruit and vegetables as well as carbohydrates . . . with dances and folksong, puppet plays and embroidery and everything that mass educators have ever thought of. I cannot see that all this is going to increase the country's economic wealth at all, unless it is claimed that a well-fed and contented miner in the Udi coalfield will raise more coal per shift than the miner of today, which may or may not be so (Vol. 2, p. 75).

Even the issue of appropriate technology appears, in the ill-fated groundnuts scheme that was undertaken in Tanganyika.

For social scientists outside the field of development studies, the history offers some nuggets of information and, in a few instances, rich veins of evidence. The course of postwar British foreign policy is the larger history into which this account must be placed. Anglo-American relations intrude from the beginning (Roosevelt, commenting on the West Indies, at his vague and whimsical best) to the end (Macmillan expressing astonishment at "anti-colonial" American opposition to a Cheddi Jagan government in British Guiana). In the 1950s constant concern is expressed about "rising demands and insufficient resources"; this is demonstrated in the management of the sterling balances, which becomes a postwar debate over colonial contributions to the war effort and in a remarkable request by Macmillan, soon after Suez, for a balance sheet of empire, "something like a profit and loss account for each of our Colonial possessions" (Vol. 5, pp. 96-97).

For specialists in organization, the history of the Colonial Development Corporation supplies a case study in the management of a public enterprise without a clearly defined task, attempting to carry out that task over a wide geographical compass. At a different level of organization, Morgan's study documents the Colonial Office obsession with schemes of federation, viewed as the only economic salvation for smaller, "non-viable" territories. The case of the West Indies (Vol. 1, Chs. 12-13; Vol. 2, Ch. 3; Vol. 4, Ch. 3; Vol. 5, Chs. 6 and 10) provides an interesting comparison with other efforts at regional organization in the developing world.

What is missing, for specialist and generalist alike, is any attention to the political context and debate that surrounded decolonization. The last volume of the history, in which Morgan deals with questions of political evolution, is thus the most disappointing. The parliamentary tumult, the bitter resistance of the settlers, the violence can only be heard in muffled form within Morgan's corridors of power. The author refers to a holding action by the "West Indian Lobby" (Vol. 3, p. 140), but the lobby is never described; "public views" are taken from the leaders of a few newspapers (Vol. 3, p. 169). The multinational corporations that played an important role in some colonial economies are absent or nearly so. The United Africa Company figures in the groundnuts scheme; a tantalizing glimpse of bargaining with these firms is provided by the case of bauxite in Jamaica (Vol. 2, pp. 112-19), but these are cameo appearances at best. The absence of politics gives this history a peculiar and distorted perspective. The most contentious and violent cases of decolonization—the strategic outposts of Cyprus and Aden and the settler colonies of Africa—are either omitted or granted a very minor place. Instead, disproportionate weight is given to the West Indies and the smaller colonial territories, of concern to the Colonial Office and development experts, but hardly central to the larger political debate.

An official history, then, a source for others, but one that gives a decidedly apolitical and antiseptic view of these years. Passion is banished from these pages until the bitter end, when the Wilson government, with some bad conscience, begins to disband the Colonial Office. A minute is produced that belies some feeling beneath the official reserve: "I must say I profoundly dislike the present trend of thinking which seems to me very like chucking a rotten carcase on the rubbish heap so that the vultures can peck its decaying flesh" (Vol. 4, p. 24). Perhaps future historians and social scientists, with access to these documents and less self-restraint, will be able to discover more of the conflict that pervaded these years.

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The Soviet Union and SALT. By Samuel B. Payne, Jr. (Cambridge, Mass.: MIT Press, 1980. Pp. 155. $19.95.)

Verification and SALT: The Challenge of Strategic Deception. Edited by William C. Potter. (Boulder, Colo.: Westview Press, 1980. Pp. xiv + 256. $26.25.)

Since the inception of the Strategic Arms Limitation Talks in 1969, a plethora of journalists and scholars have sought to define and clarify the myriad, arcane issues associated with the arms control negotiations. From John Newhouse's intimate account of SALT I in *Cold Dawn* (Holt, 1973) through Thomas Wolfe's comprehensive *The SALT Experience* (Ballinger, 1979), useful contributions to our understanding of SALT have appeared periodically. Unfortunately, despite such efforts, there remains a paucity of lengthy, objective examination of several of the most salient arms control issues. Given the overall complexity and usual emotional fervor surrounding debate on SALT topics, this inadequacy should not be surprising. This review attempts to establish the extent to which the reviewed works provide a useful and balanced contribution to the SALT literature.

Few lengthy studies have examined systematically Soviet elite perceptions of the role and utility of arms control for Soviet national security policy. That Samuel Payne's study achieves even a modicum of success in this area makes his work suitble reading for specialists and nonspecialists alike. However, for those so inclined, I must point out several methodological weaknesses. In particular, chapters 4 and 5 appear, on balance, weak and unconvincing. Here Payne presents a dichotomous view of Soviet elite perceptions of SALT; to wit, he contends that these perceptions may be divided into "arms controllers" who defend and support the SALT process and "militarists" who question SALT's worth. The data he employs to substantiate this assertion merit scrutiny. On the one hand, to his credit Payne cites numerous and varied primary sources to bolster his case. On the other hand, however, there are several limitations to this data. In fairness, I should note that the author evinces some awareness of these data limitations, but I will mention them nonetheless. First, many of the sources cited to prove the existence of "arms controllers" and "militarist" elements in Soviet political and military circles is outdated, since they first appeared in the late 1960s or early 1970s. Second, the reader should seriously question whether even the most exhaustive and current examination of open Soviet sources could provide reliable insights into the Soviet elite's perceptions of arms control. Soviet leaders, faithful to the demands of democratic centralism, are not in the habit of airing publicly substantive disagreements on policy issues. Finally, even if one concedes the existence of Soviet "arms controllers" and "militarists," given the overall thrust of both Soviet SALT negotiating strategy and arms acquisition strategy, it is problematic whether the "arms controllers" have had more than a circumscribed influence on Soviet national security policy output.

The methodological weaknesses of *The Soviet Union and SALT* should not distract the reader from the substantive points contained in later chapters. The most interesting and best-argued of the points is found in chapter 11 where the author takes up the question of whether "Soviet acceptance of arms control measures, such as strategic arms limitation, indicate any desire for any overall settlement with the West" (p. 91). The result is a solid piece of research, as Payne explains the synergistic rather than contradictory nature of Soviet foreign policy that calls at once for cooperation and conflict with the capitalist states under the rubric of peaceful coëxistence. In concluding that "the SALT agreements do not imply any reduction in the general Soviet drive to attain predominance over the United States" (p. 102), Payne is sure to encounter dissenters. Nevertheless, his analysis is based on sound understanding of Soviet ideology and the Kremlin's global adventures over the past few years lend ample support to his view.

Few informed observers would disagree with William Potter's introductory statement to *Verification and SALT,* "Verification has long been the nemesis of arms control" (p. 1). The collection of articles Potter has edited supports this assessment and, on the whole, makes an excellent contribution to delineating the major verification issues that have appeared throughout the SALT I and SALT II debates. While he does not organize them formally in this manner, the editor presents essentially three types of essays. First, several early chapters provide detailed yet clear background discussion on topics ranging from how the United States verifies SALT provisions and institutionally organizes these efforts to Soviet views on SALT verification. Notable in this group is the Blair and Brewer essay, "Verifying SALT Agreements," where the authors strike a critical note in claiming that, technical requirements notwithstanding, the fundamental issue in SALT is the level of uncertainty each side is willing to tolerate to achieve arms limitations. In other words, SALT verification contains a significant political component. Another excellent contribution in this category is Stuart Cohen's "The Evolution of Soviet Views on SALT Verification: Implications for the Future." Cohen's work is the product of diligent research and will serve, it is to be hoped, to stimulate additional studies on a topic that has received but limited attention in the United States.

Second, a group of essays presents the optimistic view of the United States' ability to verify SALT. Included under this heading is an article by SALT supporter Herbert Scoville and a Les Aspin-Fred M. Kaplan coauthored essay, "Verification in Perspective." In toto, these works present little if any additional evidence to bolster the SALT advocate's case that has not appeared in other forums. Nevertheless, the essays are well written and the points contained therein argued forcefully.

Finally, articles by Amrom Katz and William Harris take a much less sanguine view of both U.S. verification capabilities and Soviet willingness to comply with the negotiated accords. Their concerns are most disturbing, as each author cogently argues that the United States could conceivably be victimized by Soviet strategic deception and thereby exposed to the attendant political and strategic liabilities of being caught off guard. These are concerns that verification optimists are hard-put to refute.

In conclusion, for the reasons discussed above, each of the reviewed books makes a useful contribution to the SALT literature. Payne's work is moderate in tone and offers competent substantive analysis that should be of interest to observers of the Soviet system. Potter's edited study is admirably balanced and likely to become one of the premier sources on verification questions.

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Nonproliferation and U.S. Foreign Policy. Edited by Joseph A. Yager. (Washington, D.C.: Brookings Institutuion, 1980. Pp. xiv + 438. $22.95, cloth; $8.95, paper.)

This carefully crafted book stands as a tragic monument to the liberal Democratic administration that never came to be. Reading these otherwise excellent essays in Year Two of the New Cold War, one is overcome with feelings of regret for diplomatic opportunities that will not come again, and of sympathy for the authors who expended so much intellectual effort clarifying issues and concepts that have now been effectively removed from the nation's political agenda.

This unfortunate result has occurred because the Brookings Institution assembled a group of five U.S. security managers to address the issue of appropriate American response to the proliferation of nuclear weapons in the world, at the very time when the American people were electing a president who felt the spread of such weapons to be an unimportant matter and, in one specific case (Pakistan), "not any of our business."

The Brookings team was no doubt commissioned in good faith before the second phase of the Carter administration which began in July, 1979, when the president came down from the mountain to scrap the liberal initiatives of his first two years and prepare for reelection campaigning. The preliminary drafts were probably completed before the Soviet invasion of Afghanistan in December, 1979, and the final text was published (according to the foreword) shortly before the August 1980 release of Presidential Directive 59 which officially changed U.S. nuclear weapons strategy from deterrence to counterforce. Thus, even in the midst of its composition, this project was being overtaken by events.

Nevertheless, this book must also be judged within its own frame of reference. The more than 400 pages of text are of uniformly high quality, given the limited perspective of a team so removed from domestic political realities. The index is superb in its detail and usefulness; there is a glossary of sometimes obscure abbreviations and acronyms; and there are several excellent tables, particularly in the chapters concerning Japan, Argentina, and Brazil. Most importantly, unlike many edited books of readings on nonproliferation, this work makes a genuine attempt to integrate the efforts of its five highly copetent contributors.

The book is divided essentially into two sections. In the first (chapters 1-12), the prospects for nuclear weapons proliferation in five different geographical regions are assessed. General Editor Joseph A. Yager begins with Northeast Asia, specifically Japan, South Korea, and Taiwan; Richard K. Betts discusses the northern tier of Southwest Asia, namely, India, Pakistan, and Iran; Henry Rowen and Richard Brody report on the countries of the "Middle East," by which they mean Israel, Egypt, and (secondarily) a few other Arab states; William Courtney analyzes Argentina and Brazil from the South American continent; and Betts returns to cover South Africa. One small criticism: although the qualifications of most of these authors are well known, a more complete description of their backgrounds and current professional activities would be helpful.

In the second, shorter section of the book, chapters 13-18, the five authors *jointly* write their recommendations for U.S. policy in each of the five regions. A concluding chapter by Yager summarizes some general ways which the United States might influence the nuclear incentives and capabilities of the 11 nations. It is in these final 117 pages that the cross-fertilization of perspective and the attempt at integration of analysis succeeds most impressively. The overall quality and thrust of this collection thus far surpasses that of numerous other anthologies on the same subject.

What then is the cause of the fundamental disappointment and frustration that engulfs the reader of this book? Namely, that although it is the work of potential policy-advisors, no one in power today appears interested in heeding their advice. The Reagan administration, for example, has appointed to a consultative position in the Department of State Ernest W. Lefever who, in a 1979 study entitled *Nuclear Arms in the Third World: U.S. Policy Dilemma,* had long since laid the intellectual foundations for ignoring the issue of nonproliferation (see *Review* 74 [1980], pp. 587-89).

The advice offered in this book would probably be too late even if the State Department's Bureau of Security Assistance, Science, and Technology were still under the leadership of Joseph S. Nye, Jr., that most effective proponent of nonproliferation policies during 1977-79. The shift in American strategy to counterforce and the withdrawal of the SALT II Treaty from active consideration by the U.S. Senate has done significant damage to the international legal regime of nonproliferation. The link between curbing horizontal proliferation (i.e., denying nuclear weapons to states beyond the existing club of five) and stopping vertical proliferation (i.e., accretions to existing nuclear arsenals) promised in Article VI of the Non-Proliferation Treaty, has been irreparably broken. Without SALT II, the number of American strategic nuclear warheads will grow to more than 15,000 before 1985. According to Desmond Ball, in his probing analysis of post-PD 59 U.S. Nuclear Weapons Policy (*Arms Control Today,* 11, No. 2 [Feb. 1981], pp. 1-9), the potential number of Soviet military targets could go as high as 40,000. The logic of current American strategic doctrine calls for a comparable number of nuclear weapons.

Because of these trends, most of the states which are the subjects of this book—the "objects" of putative American nonproliferation policies—have long since ceased to take seriously the words of a country which talks a tough game of curbing horizontal proliferation, but whose actions in the area of vertical proliferation undercut its credibility. The U.S. arsenal of nuclear weapons had soared beyond the 9,000 mark, and that of the Soviet Union hovered around the figure 6,000, *before* the events of 1979-80. For would-be policy makers in either of these two states to sit on the sidelines and wring their hands about the possibility of India's nuclear stockpile climbing from 1 to 10, or Pakistan's from 0 to 5, or Israel's from 20 to 40, or South Africa's from 2 to 12, borders on the ludicrous.

Thus, even if Yager and Co. at Brookings were still writing for Nye and Co. at State, the objective conditions of the Soviet-American strategic competition have rendered unbelievable any case for American nonproliferation policies aimed at these 11 states. If these countries decide to forego nuclear weapons, it will be because they have learned the lessons of wasted cost and dubious utility of such an arsenal from the example of the first five nuclear powers, not because of any elaborate scheme of political disincentives, security guarantees, and technical obstacles conceived in Washington. For those states which do not make this judgment, time will have made the main points of this book irrelevant for any successor to the Reagan regime in 1985 or 1989. It is for this reason that this book fails in the most fundamental sense despite its high level of technical craftsmanship.

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